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The Political Foundations of Democracy and the Rule of Law

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This paper develops a game-theoretic approach to the problem of political officials' respect for political and economic rights of citizens. It models the policing of rights as a coordination problem among citizens, but one with asymmetries difficult to resolve in a decentralized manner. The paper shows that democratic stability depends on a self-enforcing equilibrium: It must be in the interests of political officials to respect democracy's limits on their behavior. The concept of self-enforcing limits on the state illuminates a diverse set of problems and thus serves as a potential basis for integrating the literature. The framework is applied to a range of topics, such as democratic stability, plural societies, and elite pacts. The paper also applies its lessons to the case of the Glorious Revolution in seventeenth-century England.

What accounts for the remarkable variation among states in the rule of law, a set of stable political rules and rights applied impartially to all citizens? Why are constitutional provisions easily evaded or ignored in some societies while respected in others? These questions have long been of interest to students of democracy, and the literature addressing them is immense, multifaceted, and compartmentalized. Although the literature has vastly improved our knowledge, it has not produced a general theory with well-accepted answers to the above questions.

This paper provides a new, synthetic treatment of the problem of democratic stability and the rule of law. I begin with a series of puzzles. The first concerns the absence of consensus about the role of interests and values in democratic stability. The early post–World War II literature studied the relationship between political culture and democracy (Almond and Verba 1963, Dahl 1966, Lipset 1960; see also Huntington 1968), emphasizing the role of values in maintaining democracy. Almond and Verba (1963, chapters 1, 13), for example, argue that stable democracy results from a "civic culture," a social consensus over a specific set of values. Barry (1970, 48–52), among others, turned Almond and Verba's logic on its head, arguing that stable democracy is not sustained by a set of values; instead, it engenders them. Barry used this logic to dismiss both the role of values in democratic stability and Almond and Verba's work.

In contrast to the earlier generation of scholars, most political scientists in the last fifteen years have focused on the interests and values of elites (e.g., O'Donnell and Schmitter 1986, Przeworski 1991). The elite-centric approach suggests that mass behavior and citizen values are secondary or irrelevant (Shapiro 1993). Not all modern scholars emphasize elites, however. Putnam's (1993) influential study stresses a return to the study of values with far less attention to elite behavior. Unfortunately, these disparate contributions fail to explain how elite and mass behavior fit together.

The second puzzle concerns why divided societies cause so much difficulty for democratic stability, as emphasized by Dahl (1971), Horowitz (1985), and Rabushka and Shepsle (1972). Horowitz (1985), for example, concludes that democracy has little chance in severely divided societies. Yet, as Lipjhart (1968, 1984) observes in a series of contexts, many divided societies are characterized by stable democracy (e.g., Belgium, Switzerland, mid-twentieth-century Netherlands) or nearly so (as in India).

The third puzzle concerns elite pacts, agreements among competing (and often warring) elites that initiate a transition to democracy. Although many comparativists point to them (Burton, Gunther, and Higley 1992; Karl 1986; Rustow 1970; O'Donnell and Schmitter 1986), no widely accepted approach explains precisely what problem pacts solve or what distinguishes successful from failing pacts.

The purpose of this paper is to present a new approach to the political foundations of democracy. By encompassing the three seemingly unrelated puzzles, it suggests a means for synthesizing several disparate components of the literature. The approach rests on a simple game-theoretic model of the stability of limited government. Because democratic survival requires that political officials observe limits on their behavior—for example, abiding by election results, rules governing policy choice, and a set of political rights of citizens—democracy is a form of limited government. As Przeworski (1991) argues, students of democracy have given too little attention to the issue of how democracy's limits are enforced. As shown below, an analysis of enforcement problems affords a deeper understanding of democratic stability, pacts, and democratic transition.

To study political officials' respect for limits on their behavior, I make two assumptions. First, all citizens have preferences and values about the appropriate limits on government. Second, each citizen, based on his or her preferences and values, is able to classify state actions into two mutually exclusive categories:

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legitimate actions and fundamental transgressions. Notice that I define these concepts for an individual, not for the society. No automatic mechanism is assumed to create a societal consensus about values. Citizens may have widely different views about limits on the state, legitimacy, and fundamental transgressions.

The model abstracts from the institutions of representative government by focusing on the relationship between a single political official, called the sovereign, and the citizenry. The model assumes that a necessary condition for a citizen to support the sovereign is that he not transgress what that citizen believes are her fundamental rights. To remain in power, the sovereign must retain sufficient citizen support.

To see the consequences for sovereign behavior, suppose that on a particular issue a consensus exists about the legitimate boundaries of the state and that citizens are willing to withdraw their support from a sovereign who violates them. The sovereign will avoid violating these boundaries because doing so risks losing power. The sovereign's self-interest leads him to respect limits on his behavior; that is, these limits are self-enforcing. In contrast, if citizens hold different views about limits on the state—or if they are unwilling to defend these limits—then the sovereign can violate these limits and retain sufficient support to survive. In this case, limits on the sovereign are not self-enforcing.

The approach implies that maintaining democracy is in part a coordination dilemma among citizens. Democratic stability requires that citizens agree on the limits on the state that they are to defend. Such agreement is neither natural nor automatic. Because citizens' social and economic positions vary widely, they typically disagree about the appropriate limits on government. Even if all citizens favor democracy and elections, they are likely to disagree about a range of policies, such as the political rights of dissidents, land reform, the pace of economic reform, or labor policy.

My theory suggests that self-enforcing limits on the state result when members of a society resolve their coordination dilemmas about the appropriate limits on the state. These limits include respecting not only election results but also the rules governing electoral competition and public policymaking.

The approach also stresses the importance of the interaction of formal institutions, elites, and citizens for democratic stability. Mass behavior is relevant because citizens must coordinate their reactions to prevent violations of democratic rights. Citizen reactions thus provide a component of the elite incentives. To the extent that solutions to the coordination dilemma occur, it is elites who construct them, often through pacts. And, given state institutions, elites make policy choices and choose whether or not to abide by limits on the state.

The model holds surprising power, providing a unified approach to the above puzzles. For the first puzzle, it suggests that Barry was correct to criticize the causal link in Almond and Verba (1963, chapter 13) but wrong to dismiss their association of democratic stability with a citizen consensus over particular values. In my model, the consensus is not epiphenomenal, as Barry suggested. Democratic stability occurs when citizens and elites construct a focal solution that resolves their coordination dilemmas about limits on the state. The focal solution results in a set of mass behaviors that create a "civic culture," including a consensus on values and stable democracy. The interdependence of elite and citizen behavior in the model also helps us understand the relationship between Przeworski's (1991, chapter 1) framework, with its explicit interest-based calculus, and citizen values. Although Przeworski does not discuss citizen values, I show that, when a consensus over values exists, elites face different incentives than when none exists, much as Almond and Verba and Putnam (1993, chapter 6) suggest.

Furthermore, my approach indicates that instability plagues democracy in most divided societies because ethnic divisions impede resolution of the coordination dilemma about the appropriate role of the state. This interpretation explains why democracy is often stable in the divided societies of the West, which have resolved their coordination problems. I show how Lijphart's (1968) characterization of Dutch consociationalism fits the model.

The third puzzle concerns what pacts do and how they do it. My answer is that successful pacts—such as the Glorious Revolution in seventeenth-century England, the Missouri Compromise of 1820 in the United States, or the Colombian pact of the 1950s—create a focal solution that resolves the coordination dilemmas confronting elites and citizens. To succeed, a pact must be self-enforcing, and the literature neglects this topic.

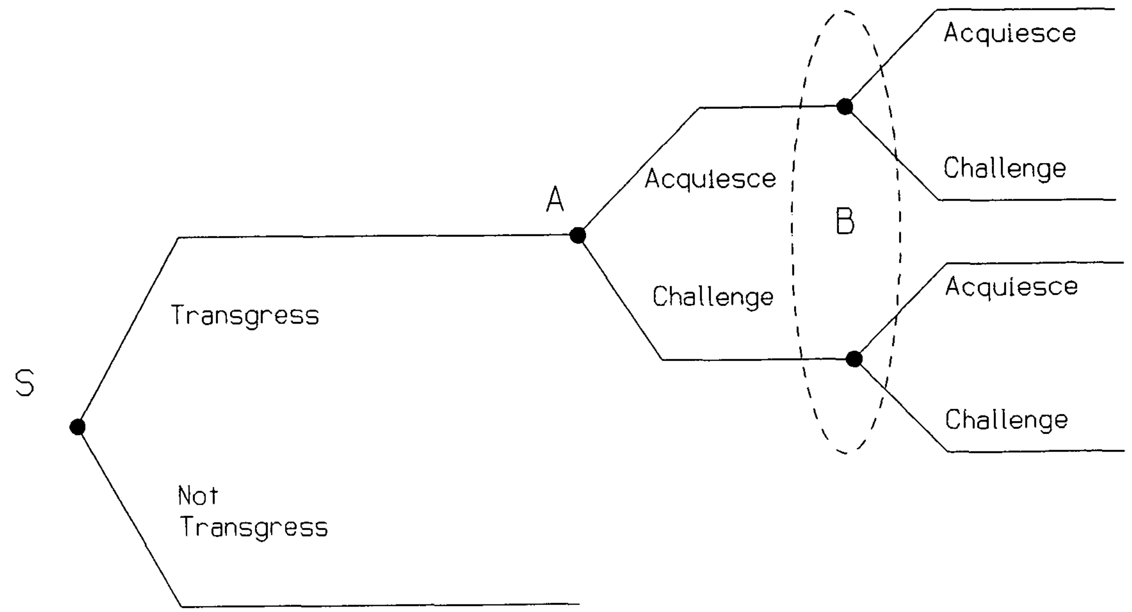
The Glorious Revolution illustrates these findings. The revolution's success, based on an elite pact, initiated a new consensus about the limits on government. Previously, widely varying notions about the state and citizen duty hindered citizen coordination. In the absence of consensus, certain transgressions against citizens went unpunished. The revolution established widely supported (at least among enfranchised elites) limits on the state. Following the revolution, the crown's respect for political limits on its behavior became self-enforcing.

This paper proceeds as follows. The next section develops the model, revealing the range of behavior that a society may exhibit, including its ability to sustain limited government. I then apply the approach to seventeenth-century England. The next section applies the model to the puzzles raised above, integrating a range of components of the literature. The concluding discussion develops the implications for the maintenance of democracy, the emergence of a civil society, and the rule of law.

THE MODEL

The model is developed in two stages, emphasizing two independent impediments to policing the state. The first stage studies the pure coordination problem induced by sovereign transgressions; the second embeds the problem of coordination in a political context.

FIGURE 1. Sovereign-Constituency Coordination Game



Model 1: Pure Coordination

I begin with a game that concerns the interaction of the polity and economy. The players are a political official or sovereign, *S*, and two groups of citizens, *A* and *B*. The economy produces the social surplus. All players share in the surplus, but the quantity produced and its distribution depend on political choices. The model also assumes that a set of economic and political rights has been specified and that these rights are compatible with economic prosperity. Whether they are enforced—and hence whether prosperity occurs in practice—depends on the interaction of the players.

The sovereign holds political power and may choose to respect or transgress citizens' rights. Power thus allows him to gain at the victim's expense, for example, by confiscating citizens' wealth. Transgressions generate an economic loss, reflecting the potential destruction of assets and the poor incentive effects generated by insecure rights. The sovereign does not hold power indefinitely, however. In order to remain in office, he needs the support of a sufficient subset of the citizens.[1] Thus, citizens hold some leverage over the sovereign in that a sufficient number of them may depose him. The model assumes that the sovereign's survival requires the support of at least one group but not both.[2]

The model's sequence of play is shown in Figure 1. *S* moves first and must choose whether to transgress citizen rights. Once *S* has chosen, *A* and *B* move simultaneously, choosing to acquiesce or to challenge the sovereign.[3] Challenging is also costly for the citizens (notice that *A* and *B* may challenge regardless of whether the sovereign has chosen to transgress). If both *A* and *B* challenge, then the sovereign is deposed, and any attempted transgression is rebuffed. If only one group challenges *S*, then the challenge fails, and the transgression succeeds. If both *A* and *B* acquiesce, then the transgression succeeds.

The payoffs from this game are given in Figure 2. Power is valuable to the sovereign, and he gains 2 from retaining it. Total social surplus is maximized when no transgressions are attempted and neither group challenges: The sovereign receives 2, and each group, 8. The sovereign values successful transgressions, increasing his payoff by 6, 3 from each citizen group. A transgression destroys half of all confiscated surplus, costing each victim 6. Challenging costs each challenger 1 regardless of whether it succeeds.

Outcomes are determined by the strategy combinations chosen by the three players. If *S* attempts a transgression and both *A* and *B* acquiesce, then the transgression succeeds, and the payoffs are: 8 to *S* (2 for retaining power and a net of 3 confiscated from each group), 2 to both *A* and *B* (8 minus the transgression's cost of 6). If *S* attempts to transgress against *A* and *B* and both challenge, then the transgression fails and the sovereign loses power, resulting in payoffs of 0

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[1] Ample evidence supports this assumption (see, e.g., Ames 1987). Even tyrannical regimes cannot survive without the support of some portion, if small, of the citizenry. As V. O. Key (1961, 3) suggests, "even in the least democratic regime opinion may influence the direction or tempo of substantive policy. Although a government may be erected on tyranny, to endure it needs the ungrudging support of substantial numbers of its people."

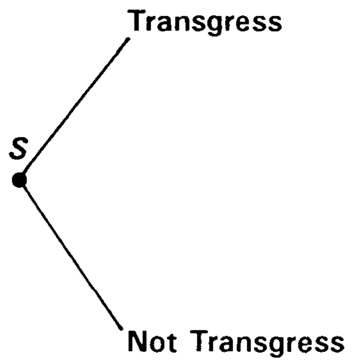
[2] This assumption does not imply that the sovereign requires the support of a majority. The groups may be of unequal size, with the support of the minority group being sufficient for the sovereign to survive.

[3] The simultaneous move between *A* and *B* is shown in Figure 1 as *A* moving first, followed by *B*, but as indicated by the dashed ellipse or "information set" around *B*'s two nodes, *B* does not know *A*'s decision when s/he must choose.

FIGURE 2. Payoffs for the Sovereign-Constituency Coordination Game

***S* Moves first**

Induced subgame between *A* and *B* (payoffs: *S,A,B*)



		B	
		Acquiesce	Challenge
A	Acquiesce	8, 2, 2	8, 2, 1
	Challenge	8, 1, 2	0, 7, 7

		B	
		Acquiesce	Challenge
A	Acquiesce	2, 8, 8	2, 8, 7
	Challenge	2, 7, 8	0, 7, 7

to the sovereign (he loses power and hence his payoff of 2) and 7 to each group (8 minus the cost of challenging, 1).

The structure of the game induces a problem of coordination among the citizens. If all act in concert, then they can prevent transgressions. If they fail to act in concert, then the sovereign can transgress the rights of citizens and survive. As in all coordination games, how one citizen group reacts to a transgression depends on how it anticipates the other citizen group will react. If the first group anticipates that the other group will challenge, then it is best off challenging. But if it believes that the other will acquiesce, then it is better off acquiescing. These two scenarios reflect the two pure strategy equilibria of the game. In one, the sovereign transgresses the rights of all citizens who acquiesce in spite of the sovereign's behavior. This is an equilibrium because of the coordination problem. Given that the other acquiesces, each group is better off acquiescing; challenging is costly and will do no good. Moreover, the sovereign has no incentive to change his behavior since he benefits from successful transgressions. Limits on the sovereign are not respected in this equilibrium.

In the other equilibrium, the sovereign honors rights in society and neither group challenges, thus maximizing social surplus. In this equilibrium, both groups challenge whenever the sovereign attempts a transgression. Given the behavior of the others, neither citizen group has an incentive to alter its behavior; nor does the sovereign. Limits on the sovereign are self-enforcing in this equilibrium.

This game reveals a natural impediment to policing the behavior of the state. Even when all members of the society agree on the definition of citizen rights, those rights may not be respected. As in all coordination games, even though all players are better off when all challenge following a transgression, they will not automatically do so.

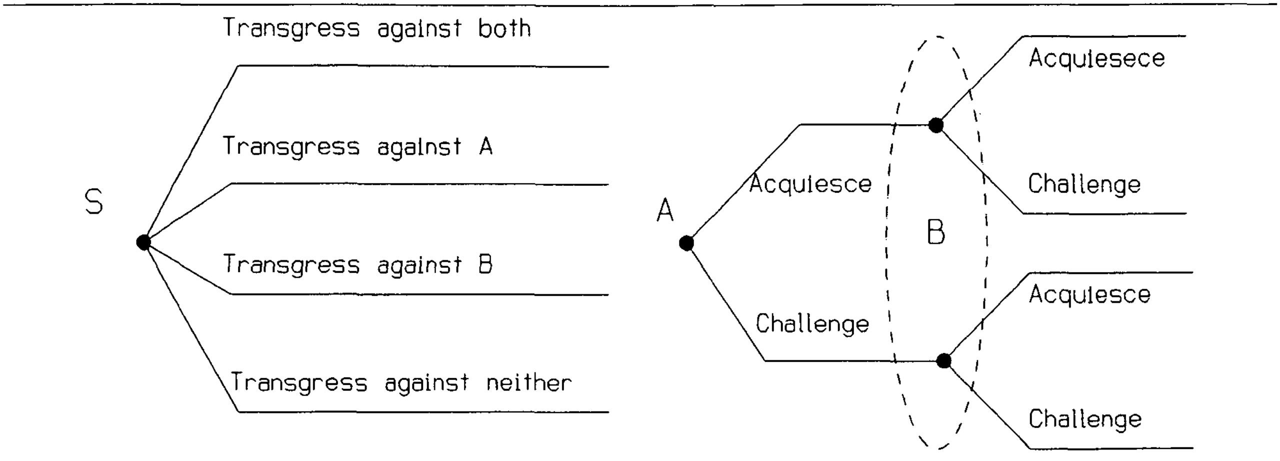
Model 2: Coordination in a Political Context

Model 1 is particularly simple. If coordination were the sole problem facing citizens, they might easily surmount it much as they overcome coordination problems, such as deciding on which side of the road to drive. Distributional issues complicate the problem considerably. Model 1 allows no differentiation between the two groups, abstracting from the diversity of opinion about the appropriate boundaries of the state and hence about what actions constitute a transgression.

Model 2 adds two political elements to the problem of transgressions. First, the sovereign need not transgress against all citizens simultaneously. Second, transgressions have distributional implications: When the sovereign transgresses against one group, he shares some of the benefits with the other group in exchange for their support. For example, violating one group's right of representation may allow the other a greater share of legislative benefits.

Specifically, *S* faces two groups of citizens, *A* and *B*; he must retain the support of at least one group in order to retain power. The sequence of action is shown in Figure 3. *S* moves first and may choose to attempt to transgress against both *A* and *B*, against *A* alone, against *B* alone, or against neither. After *S* moves, *A* and *B* move simultaneously. Each may choose to

FIGURE 3. The Sovereign-Constituency Transgression Game



acquiesce or to challenge the sovereign. Challenging is costly. If both *A* and *B* challenge, then the sovereign is deposed, and any transgression attempted by the sovereign is rebuffed. If only one group of citizens challenges *S*, then the challenge fails, and any attempted transgression succeeds. If both *A* and *B* acquiesce, then any attempted transgression succeeds.

The payoffs from this game are given in Figure 4. Social surplus is maximized when no transgressions or challenges are attempted, yielding 2, 8, and 8, respectively. Power is valuable to the sovereign, and he loses 2 if he is deposed. A successful transgression nets a total of 3. When *S* successfully transgresses against one group, he keeps 2 of the 3 and shares 1 with the other group. If *S* transgresses against both, then he keeps 6. A transgression against either group destroys half of all confiscated surplus, costing the victim a total of 6. Challenging costs each challenger 1 regardless of whether it succeeds. Outcomes are determined by the strategy combinations chosen by the three players.[4]

This game complicates the coordination problem of model 1 by adding an aspect of the standard prisoner's dilemma to the coordination problem. That aspect arises because challenging a transgression is costly. Consider the set of incentives facing each group if *S* attempts to transgress against *B*. *B* naturally prefers that both challenge. No matter what strategy *B* plays, however, *A* has a dominant strategy and will always acquiesce in the face of a transgression against *B*. Knowing this, *B* will also acquiesce.

This structure of interaction allows the sovereign to transgress some citizens' rights and survive.[5] In the one-shot game, there are three pure strategy equilibria, and the Pareto-optimal strategy combination with no transgressions is not among them. Which equilibrium occurs depends in part upon the reaction functions of the citizen groups to a transgression. The worst outcome for the citizens, where the sovereign transgresses against both, is an equilibrium. This occurs if citizens acquiesce in the face of any transgressions. The equilibrium strategies are: *S* transgresses against both *A* and *B*; *A* and *B* acquiesce under all circumstances. Neither *A* nor *B* has an incentive to deviate. Although both *A* and *B* would prefer that both challenge *S*, acting alone and taking the strategies of the others as given, neither group can change the outcome by challenging, but it will increase its costs.

The two other equilibria are asymmetric and occur when *S* targets only one citizen group. In these equilibria, citizen groups challenge *S* if and only if *both* are the targets of a transgression. The equilibrium strategies (for the case when *S* targets *B*) are: *S* transgresses against *B*; *A* and *B* challenge if and only if *S* transgresses against both *A* and *B*. None of the players has an incentive to deviate. For *S*, transgressing against both leads to being deposed (this triggers both *A* and *B* to challenge, deposing the sovereign); transgressing against *A* instead is no better; and transgressing against neither is worse. For *A*, always acquiescing yields no change in payoffs; and challenging any transgression decreases payoffs (this raises *A*'s costs without any benefit). For *B*, given that *S* targets only it and that *A* therefore will not challenge, challenging will not change the outcome but will increase *B*'s costs. Thus, the outcome of the one-shot game is particularly grim because it cannot sustain the rights of all citizens.

These equilibria can be interpreted in terms of the implicit notion of citizen duty, corresponding to how citizens respond to a transgression. For example, the asymmetric equilibrium where *S* successfully transgresses against *B* is supported by notions that citizens should respond to the most egregious violations by *S*; notably, when he targets both groups. The other equilibrium—in which the sovereign successfully transgresses against both—may correspond to the idea of

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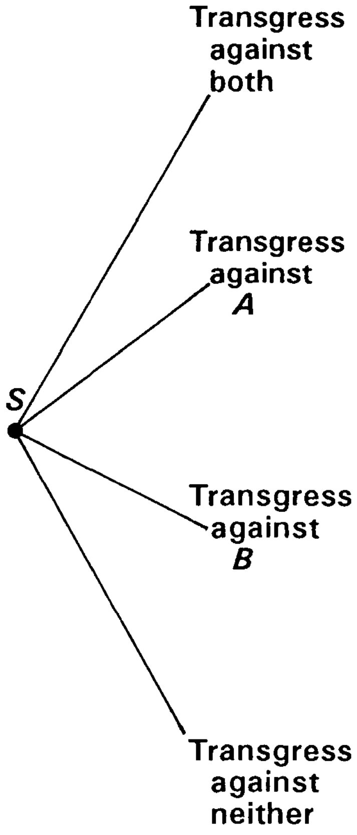
[4] For example, if *S* attempts to transgress against both *A* and *B* and both acquiesce, then the transgression succeeds, and the payoffs are: 8 to *S*, 2 to *A*, and 2 to *B*. If *S* attempts to transgress against both *A* and *B* and both challenge, then the transgression fails, and the sovereign loses power, resulting in payoffs of 0, 7, 7.

[5] The equilibrium concept is subgame perfection: A *strategy* specifies the action a player will take at every node of the game; an *equilibrium* is a set of strategies such that no player has an incentive to deviate, given the strategies of others; the equilibrium is subgame perfect if it remains an equilibrium when restricted to each subgame.

FIGURE 4. Payoffs for the Sovereign-Constituency Transgression Game

S Moves first

Induced subgame between *A* and *B* (payoffs: *S,A,B*)



Transgress against both

		B	
		Acquiesce	Challenge
A	Acquiesce	8, 2, 2	8, 2, 1
	Challenge	8, 1, 2	0, 7, 7

Transgress against A

		B	
		Acquiesce	Challenge
A	Acquiesce	4, 2, 9	4, 2, 8
	Challenge	4, 1, 9	0, 7, 7

Transgress against B

		B	
		Acquiesce	Challenge
A	Acquiesce	4, 9, 2	4, 9, 1
	Challenge	4, 8, 2	0, 7, 7

Transgress against neither

		B	
		Acquiesce	Challenge
A	Acquiesce	2, 8, 8	2, 8, 7
	Challenge	2, 7, 8	0, 7, 7

passive obedience because citizens believe the sovereign rules by divine right. Two other concepts of citizen duty cannot be supported in equilibrium: namely, that a citizen ought to respond whenever s/he is the target or that a citizen ought to respond whenever *any* citizen is the target. Both notions fail due to the dominant strategy feature of the game: When *S* transgresses against only one group, the other is always better off acquiescing.

The situation is more complicated when this game is repeated, that is, when the interaction between the sovereign and citizens is ongoing. Given the structure of payoffs, the "folk theorem" applies, implying that virtually any outcome can be sustained as an equilibrium of the repeated game (Fudenberg and Maskin 1986). In particular, each equilibrium of the one-shot game is an equilibrium of the repeated game.

Because it provides the opportunity for citizens to punish one another, repeating the game allows citizens to support the Pareto-optimal outcome, which has the following equilibrium strategies:

S: If either *A* or *B* has ever acquiesced to a transgression, transgress against both *A* and *B*. Otherwise, do not transgress.

A: If *B* has challenged every previous transgression by *S*, then challenge if *S* transgresses and acquiesce otherwise. If *B* has acquiesced to a previous transgression by *S*, acquiesce in every period.

B: If *A* has challenged every previous transgression by *S*, then challenge if *S* transgresses and acquiesce otherwise. If *A* has acquiesced to a previous transgression by *S*, acquiesce in every period.[6]

To prevent challenges by *S*, both groups must challenge any transgression. This behavior is an equilibrium under repeat play because, as in the repeated prisoner's dilemma, the players use trigger strategies to "punish" one another for failure to cooperate. For example, if *A* fails to challenge *S* when *S* attempts to transgress against *B*, then *B* can retaliate by acquiescing in the future whenever *S* attempts to transgress against *A*. This allows *S* to transgress successfully against *A*.

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[6] Technically, this equilibrium also requires a sufficiently high discount factor, δ.

When *S* attempts to transgress against *B*, *B*'s use of the trigger strategy confronts *A* with the following choice: Acquiesce today, avoiding the cost of 1, and then face losing 3 in all future periods; or challenge today, costing 1 today but maintaining 3 in all future periods. When *A* does not discount the future too heavily, it will prefer the latter. *B*'s trigger strategy induces *A* to challenge the sovereign when the latter attempts to transgress against *B* alone.

An important property of this equilibrium is that it supports a social consensus: All citizens hold the same views about transgressions and citizen duty. It thus reflects a Lockean principle of active resistance to the sovereign in the face of transgressions.[7]

Unfortunately, the Pareto-optimal outcome is not the only equilibrium. Although it is normatively attractive, this equilibrium will not inevitably occur. Among others, the game may instead yield any of the three equilibria of the one-shot game, allowing successful transgressions against some or all citizens. In these equilibria, the sovereign may transgress the rights of some citizens while retaining the support of others. These are stable patterns of behavior, and none of the players, acting alone, can alter them.

Implications of the Model for Limited Government

The sovereign-transgression game exhibits multiple equilibria. In the Pareto-optimal equilibrium, citizens police universalist limits on the state. In the asymmetric equilibria, the sovereign and one group of citizens form a coalition against the remaining citizens, transgressing the latter's rights.

Although multiple equilibria often make predictions difficult, this is not the case for the sovereign-transgression game. Because it presents members of a society with a massive coordination problem, the game's most natural equilibrium is asymmetric. This holds because most societies are unlikely to resolve the coordination problem in a wholly decentralized manner (Matsuyama 1996). People's natural diversity impedes coordination. Their economic circumstances differ considerably, including wealthy elites, successful commercial agents or economic entrepreneurs, yeomen farmers who work their own land, peasants who farm the land of others, and laborers. In addition, most individuals are members of groups that hold a range of different "cultural beliefs" (Greif 1994) and "mental models" (North 1993), including religious and ethnic groups, labor unions, and other economic, political, and social organizations. Differential economic positions, beliefs and mental models, as well as information make decentralized coordination difficult.[8]

Politics exacerbates the massive coordination problem. Because the positions and interests of citizens differ, violations of the rights of some may benefit others. For example, expropriating the wealth of specific economic groups, sectors, or other organizations—such as large landowners, laborers, agricultural workers, exporters, or the church—provides a source of funds for others. Under these circumstances, citizen views about the appropriate role of the state, about what actions constitute a transgression, and about citizen duty are likely to differ widely.

The model shows that, in the absence of a consensus about the boundaries of the state, a coalition between the sovereign and one group of citizens is stable once it is formed. Thus, the most natural equilibrium of the game is coordination failure: Citizens are unlikely to achieve coordination in a wholly decentralized manner. Typically, differential circumstances imply there is no natural focal solution to their problem.

Policing the sovereign requires that citizens coordinate their reactions, which requires constructing a coordination device. Constitutions, a charismatic leader, a galvanizing event such as a major riot, or a pact can serve to coordinate citizens' reactions so that citizens can police the state.[9] But not just any constitution or pact will do. Its provisions must be *self-enforcing*; that is, it must be in the interest of the sovereign to abide by them (Calvert 1995, Ordeshook 1992, Przeworski 1991, Weingast 1995). In terms of the model, limits become self-enforcing when citizens hold these limits in high enough esteem that they are willing to defend them by withdrawing support from the sovereign when he attempts to violate these limits. To survive, a constitution must have more than philosophical or logical appeal; citizens must be willing to defend it.[10]

Social consensus has a special meaning in this approach. The construction of a consensus about limits on the state does not imply that all citizens hold identical values, but that they agree on a set of actions that trigger their reaction. Because citizens have different views about ideal limits, a unique set of ideal limits is unlikely. Coordination requires that citizens compromise their ideal limit. Paradoxically, if all hold out for their ideal limits, citizens cannot coordinate and hence cannot police the state. When the difference between each citizen's ideal and the compromise is small relative to the cost of transgressions, compromise makes citizens better off.

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[7] Nonetheless, there is more than one way to interpret the implied citizen duty, although both are Lockean. The game-theoretic language suggests a twofold interpretation. First, citizens should react to any transgression, regardless of the target; second, those citizens who neglect this duty should be punished. Yet, the terms "punishment" and "retaliation" impute cognitive processes and are not strictly necessary. Thus, a second notion of citizen duty relies on only the first point. The behavior called "retaliation" occurs because citizens feel betrayed when others fail to come to their aid and therefore no longer feel an obligation to come to the other's aid.

[8] Matsuyama (1996) also argues that the scope of citizen differences makes it difficult for a central authority to discover the appropriate way to coordinate.

[9] This point is made with respect to constitutions by Gibbons and Rutten (1996), Hardin (1989), and Ordeshook (1992); and, in the current context, by Weingast (1995).

[10] As Learned Hand (1952, 189–90) observed in his famous essay, "The Spirit of Liberty," "liberty lies in the hearts of men and women; when it dies there, no constitution, no law, no court can save it."

As a final observation, this approach holds that democratic stability depends on both mass and elite behavior. Given state institutions, elites choose both policy and whether to abide by limits on their behavior.[11] Yet, elite choices also depend on mass behavior because the latter provides many of the incentives that elites face. For example, elites risk their future if they ignore a citizen consensus that limits on the state should be upheld and defended. When no consensus exists, elites have fewer incentives to uphold the system.

THE ENGLISH GLORIOUS REVOLUTION AS AN ELITE PACT

The Glorious Revolution in England in 1689 illustrates both aspects of the theory: an inability to maintain political rights and other limits on the state, reflecting a lack of social consensus, and the effects of constructing a coordinating mechanism to resolve these problems.[12] Limited franchise in England of the late seventeenth and early eighteenth century implied the political system was a competitive and representative oligarchy, not a democracy. Nonetheless, the analysis of this system's stability raises the same issues as analysis of democratic systems.

Prior to the revolution, considerable disagreement existed about the appropriate boundaries of the state. The revolution created a consensus about many disputed issues. The consensus was codified through various constitutional changes, that is, an elite pact. As I show, the pact was self-enforcing, providing for the consolidation of an oligarchic system of representation.[13]

The seventeenth century was one of considerable controversy and turmoil. It included a civil war, the destruction and subsequent restoration of the monarchy, the Glorious Revolution, and a coup that removed one king and brought in another. Disputes concerning the role of the state, the rights of citizens, and citizen duty occurred between both the king and citizens and different groups of citizens. By the end of the century two coalitions had formed, called Tories and Whigs. The latter were more focused on commercial activities. They favored secure property rights, low and stable taxes on economic activity, and an activist profile in international relations. They also sought explicit limits on sovereign behavior. Tories cared much less about commercial activity, wanted a low international presence, and preferred low and stable taxes on land, their primary source of wealth. They also strongly supported the Church of England and opposed explicit limits on the crown.

The two factions also differed in their views about citizen duty when the sovereign committed undesirable acts. Whigs held a Lockean view of active resistance in the face of such acts. Indeed, Locke was a Whig and wrote his famous tracts during these controversies. Arguing that sovereign power was granted by citizens, Whigs believed it could be withdrawn. Tories, in contrast, maintained a notion of passive obedience to the sovereign, believing in acquiescence in the face of undesirable acts.[14]

During the reign of the late Stuarts (from the Restoration in 1660 to the Glorious Revolution) and especially by the mid-1670s, the Tories supported the crown, whereas the Whigs opposed it. Moreover, the late Stuarts transgressed significant rights of the Whigs while retaining support of the Tories.

The most famous illustration occurred in the 1680s, when the crown initiated its campaign to "pack the constituencies," a series of transgressions against the Whigs that sought to abrogate their right of representation in Parliament.[15] This campaign proved remarkably successful.[16] By the mid-1680s, the king had violated Whigs' rights of representation while retaining the support of the Tories. The latter's self-interest, in combination with their views on the role of the sovereign and of citizen duty, led them to support the king. As long as violations of rights centered on their political opponents, the Tories benefited along with the sovereign from transgressions. Seemingly stable, this pattern might have lasted for a considerable period.

Although the motives and wisdom for what happened next have been debated for three centuries, the actions of the crown are not in dispute.[17] Following the removal of the Whigs as a formidable political opposition, the crown turned on its own constituents, using the same techniques of disenfranchisement against them. In reaction, the Tories joined the Whigs to form a united political nation against the king, forcing him to flee and inviting in a new monarchy, that of William and Mary.

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[11] Moreover, elites construct focal solutions to the coordination dilemma. Of course, in practice such elites often include representatives of citizen groups (suppressed in the model for simplicity), in addition to political officials.

[12] This section draws on North and Weingast (1989) and Weingast (1997b).

[13] Elite pacts have rarely been associated in the literature with the Anglo-American democracies (see, however, Burton, Gunther, and Higley 1992). Nonetheless, they represent critical steps in the evolution of representation and the consolidation of democracy in both Great Britain and the United States. The Magna Carta is perhaps the most famous English pact. As the discussion of the Missouri Compromise of 1820 in the next section suggests, U.S. history also reflects a series of pacts.

[14] As Miller (1992, 64) suggests: "Tories argued (conventionally enough) that, as the powers of kings came from God, resistance to kingly authority could never be justified: if a king maltreated his subjects, they should accept that maltreatment with the same fortitude as the primitive Christians under the pagan Roman emperors."

[15] A host of other instances occurred. Many political and individual freedoms were abused under the Stuarts. For example, judges were fired for not following the dictates of the crown; and excessive bail, excessive fines, and cruel and unusual punishment were all used to intimidate the political opposition. These, too, were settled following the Glorious Revolution (Schwoerer 1981, 284).

[16] Of the 104 formerly Whig strongholds recharted between 1681 and 1685, only one returned a Whig to the next Parliament (Jones 1972, 47).

[17] The details of this dispute need not concern us. In brief, for more than two centuries, Whig historians viewed the Stuarts as bumbling kings taking ill-advised actions. More recent scholarship tends to suggest that James II took an intelligent gamble that nearly succeeded. Had he won, he would have dramatically transformed the English state, emerging as one of the most powerful monarchs in Europe.

The Glorious Revolution was far more than a coup, however (Jones 1972, Schwoerer 1981). Before the new sovereign took power, the Tories and Whigs negotiated a set of institutional changes; notably, Parliament joined the crown as an equal partner in government, a transformation with significant consequences (see, e.g., North and Weingast 1989).

Most important from our standpoint are the components of the pact known as the Revolution Settlement, which Parliament passed before formally offering William and Mary the throne. This document represents a coordination device. The two sides negotiated, sometimes bitterly, over two lists: how to define the former king's transgressions and how to specify the actions that no future king could undertake without fear of reprisal. Though the participants disagreed about ideals, once a compromise occurred, they accepted it nearly unanimously.

The settlement articulated an explicit set of strategies over what actions should trigger a joint response by both groups. By creating a consensus about limits, these strategies made the agreement self-enforcing. Typical of this type of coordination problem, individuals disagreed about what events should trigger a reaction, but resolution of that coordination problem brought unanimous support for the trigger strategies:

> The thirteen points in the Declaration were not just statements of the true nature of the law of the constitution, *they were also intended to provide a guideline for the future conduct of government, so that any departure from legality would be instantly signaled, and remedial action could be taken* (Jones 1972, 318, emphasis added).

The constructed consensus also altered notions of citizen duty. Tories relinquished the doctrine of passive obedience in the face of sovereign transgressions. No longer would the king appeal to divine right to rationalize his decisions and seek obedience from his subjects.[18]

The consensus after the Glorious Revolution was thus a direct consequence of the new set of "pacted" institutions. It did not result from a new, uniformly held ideal about the best form of English government. The agreement instead occurred over the appropriate trigger strategies coordinating citizen reactions. For many of the most central political issues of the era, the Revolution Settlement furnished a new set of limits on sovereign behavior. The newly agreed-upon limits implied that citizens would react in concert against any future violation of them, thus making their political and economic rights more secure. Because violations of these limits would now be punished, the crown had a substantial incentive to observe them. The pact thus made the limits on the state self-enforcing.

DEMOCRATIC STABILITY

This section applies the approach to the three puzzles noted in the introduction. It also shows how to integrate other components of the literature, notably the study of democratic consolidation.

Values, Interests, and Democratic Stability

The first puzzle concerns the relationship between citizen values and elite interests in democratic stability. In the early literature, scholars focused on citizen attitudes and values. Almond and Verba (1963) argued that citizens in stable democracies are characterized by a particular set of widely shared attitudes and values, which they called the "civic culture."[19] Citizens in stable democracies possess a relatively common set of understandings about the appropriate boundaries of government, the sanctity of political rights, and the duties of citizens to preserve them: "If there is no consensus within society, there can be little potentiality for the peaceful resolution of political differences that is associated with the democratic process" (Almond and Verba 1963, 358).

The equilibria of the game correspond closely to the phenomena studied by Almond and Verba. Societies that have resolved their coordination dilemmas, paralleling the Pareto-optimal equilibrium, exhibit three complementary phenomena: stable democracy, a set of political institutions and rights of citizens that define limits on the state, and a shared set of beliefs among the citizenry that those limits are appropriate and worth defending. Societies that have failed to resolve their coordination dilemmas, paralleling the asymmetric equilibrium, lack these complementary phenomena.

The model suggests, however, that stable democracy does not simply arise because some countries happen to have the relevant shared set of values. As Barry (1970, 48–52) observed, stable democracy may instead foster citizen values.

The relationship between citizen values and democratic stability is not a causal one, with values as the independent variable and democratic stability as the dependent variable (Lijphart 1980); nor, as Barry suggested, is it simply the reverse. My approach suggests a third alternative in which the causal variable is whether a society has resolved its coordination dilemmas. Resolution creates both a consensus among the citizenry and stable democracy; they are two different aspects of the same equilibrium. Failing to resolve the dilemma implies a diversity of citizen attitudes and the lack of democratic stability.

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[18] To quote Schwoerer (1981, 291): "The events of the revolution and the terms of the Bill of Rights destroyed the essential ingredients of the ancient regime: the theory of divine-right monarchy, the idea of direct hereditary succession, the prerogatives of the king over law, the military, taxation, and judicial procedures that were to the detriment of the individual."

[19] See also Dahl (1966, 1971) and Lipset (1960, 1963). The discussion emphasizes only one aspect of this rich literature. It also suggests a way to integrate some of the more recent and parallel findings of Putnam (1993).

Almond and Verba provide considerable support for my interpretation of their work. Paralleling my view, they argue that citizens in stable democracies have the ability to cooperate: "Social trust facilitates political cooperation among the citizens in [stable democracies], and without it democratic politics is impossible" (1963, 357). My account emphasizes the consensus about the appropriate limits on government. For Almond and Verba (p. 357), the norms underlying the civic culture "place limits on politics. They indicate that certain social relationships are not to be dominated by political considerations. And in this way they allow the individual to maintain a certain degree of independence from the political system."[20]

But how does the civic culture constrain political officials? My equilibrium perspective concludes that officials respect limits because they fear being deposed or turned out of office. Although Almond and Verba (1963, 353) are not centrally concerned with this issue, they point to mechanisms that parallel those of my model:

> The well-known "law of anticipated reactions" may operate here. A good deal of citizen influence over governmental elites may entail no activity or even conscious intent of citizens. On the contrary, elites may anticipate possible demands and activities and act in response to what they anticipate. They act responsively, not because citizens are actively making demands, but in order to keep them from becoming active.

In sum, my interpretation of Almond and Verba seems in sympathy with their own approach.

Contrasting Illustrations of Citizen Behavior. The contrast between political behavior in a stable democracy like the United States and in the historically unstable ones of Latin America illustrates the differences in citizen behavior implied by the model. A revealing example from the United States concerns the citizen reaction to President Franklin Roosevelt's 1937 proposal to "pack the Court." In the previous two years, the Supreme Court had declared many of the New Deal's principal components unconstitutional. In response, Roosevelt devised a plan to expand the Court to obtain a pro–New Deal majority, but he never pressed for it. Among the reasons—and critical for my purposes—was the public reaction to this plan. Not only his political opponents but also many of his supporters were against the plan. Because it constituted a direct assault on the constitutional principle of the separation of powers, large numbers of citizens, including many of the intended beneficiaries, viewed the plan as illegitimate.[21]

This type of citizen reaction is significantly less likely in Latin America. Many of these states are characterized by cycles of democracy and authoritarianism, which constitutes a fundamental difference between democracy there and in the United States. Latin American states are not characterized by a common set of citizen attitudes about the appropriate role of government. When the government threatens radical reform, or after extended poor economic performance due to political mismanagement or corruption, a surprisingly large portion of the citizenry has in the past been willing to support extraconstitutional means of political change. These findings typify the literature on democracy in Latin America. Stepan's (1988) analysis of Brazil suggests that, in the 1964 coup, the military moved only after it sensed considerable support for its action. Baloyra (1986), in his study of Venezuela, found that in 1983 more than half the respondents (53%) could conceive of situations in which military coups are justified. Loveman (1993) argues that most Latin American constitutions facilitate coups and other forms of "regimes of exception," for example, through provisions that charge the military with maintaining public order.

These examples illustrate one of the central results of the model: Maintaining limits on the state requires that political leaders find it in their interests to abide by them. The U.S. Constitution has proved binding in practice partly because citizens are willing to defend it by reacting against proposed violations. Anticipating that reaction, political leaders rarely attempt violations. Citizen reaction implies that U.S. constitutional restrictions on elected officials are self-enforcing. In contrast, the findings about Latin America reveal a substantial degree of support for extraconstitutional action during periods of poor economic and political performance.[22] Paralleling the equilibrium of the game studied above, Latin American states exhibit a complementary set of phenomena: citizens unwilling to defend the constitution, unstable democracy, and episodic support for coups. Democracy is self-enforcing in the United States but not in Latin America.

Citizen attitudes, values, and behavior are thus central to democratic stability. But they are not a set of causal variables, as Almond and Verba suggested. Along with democratic stability, citizen behavior reflects the equilibrium; and both phenomena are determined by whether the society has resolved its coordination dilemmas.

Values and Interests in the Recent Literature. Uncertainty over the relative importance of interests and values continues to the present. Whereas the 1960s literature tended to focus on mass behavior, ignoring elites, students of democracy in the 1980s exhibited the opposite tendency (as exemplified in the famous study by O'Donnell and Schmitter [1986]). Two of the most influential works of the 1990s illustrate the ongoing lack of consensus. Przeworski's *Democracy and the Market* (1991) focuses on the interaction of institutions and elite incentives; citizen values and behavior play no explicit role. In contrast, Putnam's *Making Democracy Work* (1993) reflects the return to citizen values, norms, and traditions, with far less attention to elites. I discuss these works in turn.

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[20] As a final parallel with my approach, Almond and Verba (1963, 372) hint at the importance of constructing a focal solution in the creation of stable democracy: "If a new nation is to create a civic culture . . . [t]here must be a symbolic event, or a symbolic, charismatic leader, or some other means of creating commitment and unity."

[21] As one prominent legal historian concluded, "the court-packing plan was attacked from all sides as a threat to the independence of the justices and to the whole American system. The plan was hastily abandoned" (Friedman 1984, 188). In striking contrast to the overwhelming support for Roosevelt and the New Dealers in Congress in the November 1936 elections, data from opinion polls suggest that at no time during the controversy did a majority of the country favor this plan (Epstein et al., 1994, table 8–26).

[22] Remmer (1991) argues that citizens in Latin America have become less willing to support extraconstitutional action since 1980.

Przeworski's (1991) approach to self-enforcing democracy contributes fresh insights into democratic stability. My framework helps reveal the logic underlying Przeworski's approach; it also reveals a hidden role for citizen values. Przeworski begins with a fundamental dilemma for democratic stability: Why would an incumbent party that has lost an election accept its loss instead of subverting the democratic process to retain power? His answer is that sustaining democracy requires it to be self-enforcing; that is, it must be in the interests of the incumbents to accept their loss.

Let the incumbent's payoffs associated with winning, loss, and subversion be W_t, L_t, and S_t, respectively (the subscripts indicate time period).[23] After losing an election in period 1, an incumbent party has two options: It may accept its loss, or it may attempt to subvert the democratic process to retain power. For there to be a potential compliance problem, the value of subversion must exceed that of accepting today's loss, or $S_1 > L_1$.

First-period payoffs favor subversion since $S_1 > L_1$. Yet, democratic institutions provide the opportunity for winning in period 2. If the defeated incumbent complies in this round, its expected payoff from the next round is $C_2 = pW_2 + (1\text{-}p)L_2$, where p is the probability that it wins the next election. Today's losers will comply when the expected gains from accepting the loss exceed those from subverting; that is, when the following inequality is satisfied:

$$L_1 + C_2 > S_1 + S_2. \qquad (1)$$

I derive two comparative statics results from this inequality about compliance and democratic stability. Both provide the logic underlying observations made by Przeworski about characteristics of successful constitutions. The first concerns Przeworski's (1991, 36) statement that "constitutions that are observed and last for a long time are those that reduce the stakes of political battles."

To see how this conclusion follows from inequality 1, rewrite the inequality so that the first term is a function of a parameter, α:

$$L_1(\alpha) + C_2 > S_1 + S_2.$$

The term α reflects the degree of constitutional restrictions on the stakes, where higher levels of α correspond to greater restrictions. Following Przeworski, assume that greater restrictions lower the stakes of political battles, that is, $\partial L_1/\partial\alpha < 0$.

But how are constitutional restrictions enforced? Although Przeworski does not analyze this problem, the above framework provides an answer. Constitutional restrictions impose limits on elected officials. The model shows that enforcing these limits requires a citizen consensus that these restrictions are worth defending. Consensus about limits implies a higher α. Higher α lowers L_1 and, by inequality 1, makes compliance more likely.

Przeworski's (1991, 36) second observation is:

> successful democracies are those in which the institutions make it difficult to fortify a temporary advantage. Unless the increasing returns to power are institutionally mitigated, losers must fight the first time they lose, for waiting makes it less likely that they will ever succeed.

The institutions of successful democracies limit the ability of those in power to subvert the system to prevent their opponents from winning the next election. In terms of the formula for compliance (1), these institutions prevent tomorrow's incumbents from substantially lowering p. Low values of p imply C_2 is near zero, so inequality 1 fails to hold, making compliance by today's defeated incumbents unlikely.

Honoring institutional restrictions must be in the interests of tomorrow's incumbents, in order to have the intended effect of compliance. My framework suggests that restrictions on governing elites can only be binding if there exists a citizen consensus to react against tomorrow's incumbents if they attempt to rig elections. Citizen consensus implies a higher C_2 and, by inequality 1, a greater likelihood of compliance.

The comparative statics results derived from inequality 1 reveal the logic underlying Przeworski's important conclusions. Democratic stability depends in part on elites' willingness to observe restrictions on their own behavior. These results also show that citizen values are a direct and explicit part of the elite's interest calculation. Przeworski argues that successful constitutions reduce the stakes of power and prevent incumbents from rigging elections against their opponents. To succeed, both aspects of constitutions must be self-enforcing. That, in turn, requires the citizen value consensus noted in the earlier literature.

Putnam's (1993) study represents a major contribution to our understanding of democracy and a return of interest in traditions, norms, and values. Putnam argues that democratic stability depends on a specific form of social organization and citizen values, which he calls "civic traditions."

My account of democratic stability parallels that of Putnam in several respects. He lodges his view in a series of "collective action problems" that focus on the ability of citizens to cooperate for mutual gain (chapter 6). He repeatedly refers to the prisoner's dilemma for understanding social cooperation (chapter 6). Societies with the appropriate norms and traditions can sustain cooperation, as reflected in northern Italy, where these traditions include strong social norms, such as reciprocity, and dense "networks of civic engagement" (chapter 6).[24] The denser are such networks and norms of reciprocity in a society, "the more likely that its citizens will be able to cooperate for mutual benefit" (p. 173). Less civic regions, in contrast, reflect a "Hobbesian equilibrium" characterized by "mutual distrust and defection, vertical dependence and exploitation, isolation and disorder, criminality and backwardness" (p. 181). Finally, Putnam describes the contrasting patterns of northern and southern Italy as "two equilibria" (pp. 177–81).[25]

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[23] Notice that S_t reflects the uncertainty associated with attempted subversion: the value of success times the probability of success balanced by the value of failure (along with any punishment) times the probability of failure. Following Przeworski (1991), for simplicity the analysis considers only two periods.

Missing from Putnam's account is a full integration of elites. Although he suggests that citizen cooperation constrains political officials to respect citizen interests, he does not analyze this problem. My perspective suggests how to complete Putnam's model, a reinterpretation that seems consistent with his account. Putnam (1993, 171–76) suggests that vertical social relations, such as patron-client networks, do not have the same political efficacy as the horizontal social relations of civic traditions.[26] He suggests that vertical social phenomena be modeled as a prisoner's dilemma in which an all-powerful state subjugates all citizens. This is a useful metaphor but a misstep toward modeling. The equal oppression in the prisoner's dilemma reflects a virtually horizontal, not vertical, society.

The key to modeling vertical social systems is that they reflect social differentiation. Some citizens are better off in the vertical society, in part because political officials reward collaboration in their subjugation of others. My model encompasses precisely this situation. In the asymmetric equilibrium, the sovereign gains the support of some citizens while transgressing against others. The asymmetric treatment of citizens underpins the equilibrium's stability, preventing sufficient support for the universalistic standards of a horizontal, democratic society.

Putnam's study of civic traditions is rightly regarded as a major work. My reinterpretation suggests how to integrate his approach with works focusing on elites.

To summarize this subsection: My model suggests that understanding democratic stability requires three components: analysis of institutions, citizen values and behavior, and elite behavior. To quote Diamond's (1994b, 3) elegant summary of the logic: "Elites choose democracy instrumentally because they perceive that the costs of attempting to suppress their political opponents exceed the costs of tolerating them (and engaging them in constitutionally regulated competition)." Studying elites or citizen behavior alone is insufficient to explain why democracy works.

Democratic Stability in Divided Societies

The second puzzle concerns why divided societies face particular obstacles to maintaining democracy (Horowitz 1985, Rabushka and Shepsle 1972). The perspective developed above provides new insights into this question. In divided societies, members of different ethnic, linguistic, religious, or racial groups typically have different views about all aspects of government, policy, and the role of the state, including the appropriate limits on the state. These differences impede resolution of the society's coordination dilemmas. Severely divided societies, those with significant ethnic conflict, therefore reflect the equilibrium in which there is an absence of shared beliefs about the appropriate boundaries of the state.

Consistent with this, Rabushka and Shepsle (1972, 12) emphasize that severely divided societies typically "lack consensus" and are states in which subnational cultural groups—as opposed to the nation—serve as the primary basis of citizen loyalty. They find that "plural societies are qualitatively distinct from homogeneous ones . . . [and] that plural societies are inherently prone to violent conflict" as opposed to peaceful, democratic resolution of their differences. Divided societies are thus less likely to sustain stable democracies. Horowitz (1985, 8), in his classic treatise, describes divided societies in similar terms: "Issues that elsewhere would be relegated to the category of routine administration assume a central place on the political agenda of ethnically divided societies . . . almost any issue, any phenomenon, can suddenly turn ethnic or turn communal."

When ethnic divisions permeate politics, the consensus necessary for democratic stability is difficult to devise.[27] The divided societies studied by Horowitz and Rabushka and Shepsle therefore reflect the asymmetric equilibrium of the game in which citizens fail to coordinate their behavior. Universalistic limits on government, applying to members of all ethnic groups, are difficult to sustain for several reasons. First, significant differences in values among members of different groups impede the resolution of the coordination problem to achieve consensus over fundamental rights and limits on the state. Second, as in the asymmetric equilibrium of the game, one group may actually benefit from exploiting others. Third, even when all groups desire an end to repression, it may be difficult to devise a credible plan or pact that implements peace. These factors make democracy difficult to sustain in plural societies. The absence of credible limits on the state allows mutual hostility to erupt into violent conflict (Weingast n.d.).

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[24] A further parallel: For Putnam (1993, 172), "in communities where people can be confident that trusting will be requited, not exploited, exchange is more likely to ensue. Conversely, repeated exchange over a period of time tends to encourage the development of a norm of generalized reciprocity." In the Pareto-optimal equilibrium of my game, one group aids another that is targeted by the sovereign partly because it expects reciprocity, not immediate gain. In contrast, citizen groups exploit one another under the asymmetric equilibrium. Finally, both the Pareto-optimal equilibrium of my game and Putnam's reciprocity are enforced through sanctions.

[25] In southern Italy, for example, "the strategy of 'never cooperate' is a stable equilibrium, for reasons that are well explicated in standard accounts of the prisoner's dilemma. . . . The 'amoral familism' that Banfield observed in the Mezzogiorno is, in fact, not irrational, but the only rational strategy for survival in this social context" (Putnam 1993, 177).

[26] "A vertical network, no matter how dense and no matter how important to its participants, cannot sustain social trust and cooperation" (Putnam 1993, 174).

[27] Recent work emphasizes that linguistic, ethnic, and religious divisions need not be the primary basis for political organization or an individual's primary loyalty (e.g., Bates 1983, Horowitz 1985, Laitin 1988). Indeed, Laitin's work (1988, 1992, 1994) on language reveals that these patterns are endogenous. The literature on nationalism, stressing the growing homogeneity of political loyalties in many societies, supports the same conclusion (e.g., Anderson 1991, Gellner 1983).

Nonetheless, as Fearon and Laitin (1996) emphasize, not all divided societies exhibit ethnic conflict of the type described by Horowitz.[28] Several divided societies in the developed West, such as Belgium and Switzerland, have developed stable democracies, and the reason underlying this stability is instructive. Those societies have devised a set of constitutional provisions to limit the effect of ethnic and religious divisions.[29] In terms of the model, these institutional provisions reflect a solution to the coordination dilemma, constructed so that limits on the state can be sustained in equilibrium. In the presence of credible limits on the state, ethnic groups can trust one another and support mutual tolerance.

The solution to the coordination problem is accomplished through a variety of institutional means, such as the form of electoral system, the decentralization of political power to more homogeneous political units, and the imposition of explicit limits on majorities at the national level.

> Both Belgium and Switzerland have a written constitution: a single document containing the basic rules of governance . . . [that] can only be changed by special majorities. . . . [In Belgium] any bill affecting the cultural autonomy of the linguistic groups requires not only the approval of two-thirds majorities in both chambers but also majorities in each linguistic group (Lijphart 1984, 29–30).

Both the decentralization of political power and explicit limits on majorities decrease the likelihood that one ethnic or religious group will use political control to discriminate against or subjugate another. As Lijphart (1984, 23) emphasizes, in "plural societies, . . . majority rule spells majority dictatorship and civil strife rather than democracy."

Lijphart's (1968) characterization of the "politics of accommodation" in the Netherlands suggests one way to construct credible guarantees to ethnic or religious groups, at least in the developed West.[30] Identifying four political subcultures, he describes a divided society in which the "fourfold division of Dutch society is manifested in virtually all politically and socially relevant organizations and group affiliations" (1968, 23).[31]

Despite the permeative divisions in Dutch society, democracy and limited government remained relatively stable during the period studied. The reason, according to Lijphart, is not due to a consensus over fundamental values or appropriate public policy. Instead, the consensus encompasses the rules of the game, which provide significant autonomy for each group. According to Lijphart (1968, 78):

> In the Netherlands, both the degree and extent of political consensus are very limited but one vitally important element of consensus is present: the desire to preserve the existing system. Each bloc tries to defend and promote its own interests but only within the confines of the total system and without the threat of secession or civil war.

The rules of the game include a series of unwritten, informal, and implicit tenets. From our perspective, the most important are: (1) The blocs agree to disagree; (2) politics is not like warfare in that no group should attempt to dominate or repress another; and (3) large numbers of politically divisive issues should be settled so that resources are distributed proportionate to the size of the relevant groups (Lijphart 1968, chapter 7).

In terms of my model, the stability underlying the "politics of accommodation" reflects three factors. First, it reflects a consensus over the rules. Citizens agree that the rules must be defended and that appeals to violate them must be opposed, even by the intended beneficiaries of the violation. Second, the proportionality features of the system, though arbitrary, represent a constructed focal point from which deviations are easy to police. Third, in accordance with my theory, stability depends on the interaction of informal norms and formal political institutions.

As described by Lijphart, midcentury Dutch democracy remained stable because of the consensus supporting the system. Elites did not attempt subversion of democratic rules because citizens, aware of the consequences, would punish leaders who advocated it. The consensus-producing aspects of the system, fostered in part by the proportionality constraints, implied that no group would be cut out of the system. Proportionality thus contributed to democratic stability because it lowered the stakes of political action.

Taken together, these arguments imply that the system's constraints were self-enforcing.[32] Despite the potentially divisive nature of this society, it achieved democratic stability.

Where divided societies have risen above their divisions, they do so by constructing explicit and self-enforcing constraints on the government that protect the various groups. The construction of self-enforcing limits not only supports mutual tolerance and trust among groups but also is what distinguishes the relatively peaceful divided societies in the West from the deeply divided societies of Africa and Asia. Maintaining self-enforcing limits on the state in divided societies requires more than just granting equal rights to all citizens. It requires institutions that provide for credible limits on the state. In both Belgium and Switzerland, power is decentralized to more homogeneous groups; in addition, national majorities are prohibited from imposing uniform policies.

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[28] The material in this subsection draws on Weingast (n.d.). Although Horowitz (1985) attempts to distinguish divided societies in the West from those he studies, these differences are largely behavioral. Because behavioral patterns are endogenous, they are part of the phenomena to be explained, not a set of independent variables that distinguish among societies. As I suggest below, these behavioral differences arise because divided societies in the West have resolved the problem of ethnic cooperation, not because ethnic cooperation is less of a problem in the West.

[29] See Lijphart (1984) and Tsebelis (1990, chapter 6.).

[30] My discussion of consociationalism in the developed West is *not* intended to suggest that it is a recipe for democracy in divided societies of the developing world, where its effects remain uncertain (see the debate between Horowitz 1991, especially his discussion of Lijphart). Instead, I suggest that the major lessons of self-enforcing limits on government are central to the success of consociationalism in the Netherlands.

[31] Furthermore, cleavages are not "cross-cutting" but "mutually reinforcing" and "congruent." "Class and religious cleavages separate self-contained 'inclusive' groups with sharply defined 'political subcultures' " (Lijphart 1968, 14–5).

[32] Of course, the long-term success and stability of constraints and institutions of this type are vulnerable to significant changes in the composition of groups, a factor influencing the stability of Dutch politics, as well as that in post–World War II Lebanon and in the accommodation of the North and South in the antebellum United States (see Weingast 1997a).

This perspective yields a prediction. Consider divided societies in the regions studied by Horowitz (1985) that have managed to secure a degree of ethnic harmony under conditions of ethnic tension, such as Malaysia or India. The above argument predicts that these societies are characterized by institutions that provide incentives for political leaders to encourage mutual tolerance and by a citizen consensus that wants these institutions preserved. Horowitz's (1985) discussion of electoral systems, for example, in Nigeria, and his (1991) discussion of South Africa in part reflect this logic.

Elite Pacts

The third puzzle concerns elite pacts (Burton, Gunther, and Higley 1992; Karl 1986; O'Donnell and Schmitter 1986; Rustow 1970). Pacts represent an agreement among elites to modify the rules of the political game, typically to end debilitating conflict.[33] According to Karl (1986, 198),

> pact-making promulgates regime norms and state structures that channel the possibilities for economic change in an enduring manner. In Venezuela ... pacts [establish] political "rules of the game" which also institutionalize the economic boundaries between the public and private sectors, guarantees for private capital, and the parameters of future socioeconomic reform.

Under certain circumstances, pacts enable a society to move from the undesirable, nondemocratic equilibrium to the desirable equilibrium of limited government that can support stable democracy.

How is this accomplished? The literature does not provide a systematic answer; nor does it explain why some pacts succeed and others fail. My approach suggests an answer, namely, that the central role of a pact is to construct a focal point about limits on the state. That requires a compromise among elites and their followers in which each group is willing to forgo attempts to dominate or subjugate other groups in exchange for mutual acceptance of a compromise. Pacts are thus a form of elite convergence that allows a previously " 'disunified' elite [to] become 'consensually unified' in regard to the basic procedures and norms by which politics will henceforth be played" (Burton, Gunther, and Higley 1992, xi).

The model suggests that successful pacts meet two conditions: (1) The participants all perceive that they are better off under the pact than under the status quo (as Rustow 1970 suggests); and (2) the pact is self-enforcing, requiring in part that the participants resolve their coordination dilemmas about the nature of the state and society. In particular, elites and their followers must be willing to punish those who seek unilateral defections from the pact. For a group to forgo opportunities to defect, it must believe that other groups will also do so. The concept of self-enforcement provides an additional set of constraints inadequately investigated in the literature.

The interpretation of pacts as constructing a focal equilibrium yields two predictions. First, pacts cannot be imposed at just any time. The model's asymmetric, nondemocratic equilibrium is stable in part because one of the two groups gains by subjugating the other. Because this is an equilibrium, something outside the model must dislodge it. For example, an economic crisis may imply that the status quo can no longer be sustained; the state may become mired in an unresolvable civil war; or a new foreign threat may provide an incentive for groups to forgo their previous differences. Consistent with this prediction, Burton, Gunther, and Higley (1992, 14) observe that crises and continual conflict often create a situation in which "all factions suffered heavy losses" (see also Rustow 1970). External events that dislodge the old pattern afford an opportunity to construct a focal point, allowing factions to implement the Pareto-optimal equilibrium sustaining limited government and democracy.[34]

The second prediction concerns the success of pacts and hence the survival of democratic institutions inaugurated by them. The model predicts that successful pacts must be self-enforcing. For an appropriately constructed sample of pacts, we should find that those which provide the basis for their own enforcement are more likely to succeed than those which do not. Studies of pacts should routinely investigate their self-enforcing properties. The literature has tended instead to focus on characterizing successful and unsuccessful pacts; for example, noting that successful pacts protect each participant's "vital interests" (O'Donnell and Schmitter 1986, 37). But why are only some pacts successful at this? My answer is that they are self-enforcing.

A final point is that self-enforcing pacts are sometimes impossible to devise. The economics literature demonstrates that incentive problems, such as ex post opportunism, plague the success of many potential bargains (Milgrom and Roberts 1992, Williamson 1985). Some bargains may fail, not because the parties are unable to reach an agreement about what to do, but because they cannot make the agreement self-enforcing. If scholars begin to investigate the incentive problems facing potential pact makers, I predict they will discover similar impediments for pacts.

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[33] In the words of O'Donnell and Schmitter (1986, 37), an elite pact is "an explicit, but not always publicly explicated or justified, agreement among a select set of actors which seeks to define (or, better, to redefine) rules governing the exercise of power on the basis of mutual guarantees for the 'vital interests' of those entering into it."

[34] Although the model focuses on the asymmetric and the Pareto-optimal equilibria, nothing in the approach requires that pacts move a society to the Pareto-optimal equilibrium. Other equilibria exist where, as Karl (1990) observes, pacts benefit only some contending interests while excluding others.

Two illustrations reveal the centrality of enforcement to pacts. Wood's (1995) study of the recent El Salvador peace accords analyzes an attempt to construct a self-enforcing pact. Although the pact may not succeed in maintaining democracy, it did end the civil war and bring on democratic elections. The analysis shows that the participants were centrally concerned with how to enforce the pact. A series of considerations facilitated compromise. After years of war, both the insurgents and the regime recognized that neither could win. The war also dramatically altered the pattern of landholdings in many insurgent areas. Because the new landholdings could not easily be undone, insurgent holdings had a degree of protection. The insurgents gave up their goal of socialist revolution, advocating democracy instead. And moderates prevailed in the regime, opening negotiations with the insurgents and agreeing, for instance, to moderate social and economic reform.

To initiate peace and transition to democracy, the pact had to solve several credibility problems. One concerned security: While peace required that the insurgents lay down their arms, if they did so, they risked having the government renege on the agreement by subjugating them. Demobilization became the Gordian knot preventing peace (Wood 1995, 235).

Two accords in late 1991 and early 1992 attempted to resolve these problems. The accords created a linked, sequential, and simultaneous demobilization in which both sides undertook a series of steps: (1) They laid down their arms; (2) the regime dismantled its internal security apparatus, formerly focused on coercing rural *campesinos*, from whom the insurgents drew their political strength; (3) the police forces were integrated; and (4) a body of observers, domestic and international, was created to help monitor and implement the agreements. Also important was the U.S. withdrawal of funds for the regime's fight against the insurgents. "This linkage thus provided some degree of security to both sides: each was required to take a sequence of significant and costly steps which, if implemented, would signal continuing compliance with the terms of the peace agreements" (Wood 1995, 238).

Although the pact provided for peace and democracy, we do not yet know whether it is stable over the long run. Many divisive social and economic issues have yet to be addressed. Nonetheless, the sides' attempt to establish credibility was central to the peace accords. The simultaneous demobilization and integration of police forces lowered the insurgents' vulnerability and thus helped facilitate peace and the transition to democracy.

A second illustration concerns the U.S. pact commonly known as the Missouri Compromise of 1820 (Weingast 1997a). It ended the first crisis over slavery by establishing sectional balance, the equal representation of North and South in the Senate, granting both sections a veto over national policymaking.

During the first two decades of the nineteenth century, many northerners, particularly traders in the Northeast, were highly dissatisfied with national policy. Throughout this period, the North retained parity with the South or had a one-state advantage. The attempt to admit Missouri as a slave state in 1819, granting southerners a one-state advantage without any free state for balance, mobilized large numbers of northerners against the South. Threatened by potential southern dominance of the Union, northerners met this threat by attacking slavery. In the House of Representatives, where population advantage gave them a majority, they amended the statehood bill to prohibit slaves from entering Missouri and to provide for a gradual emancipation of slaves already residing there (Moore 1953). Southerners, with their veto in the Senate, prevented the amendments from becoming law, and a crisis ensued.

Because a national antislavery initiative struck at the heart of southern society, the crisis had a larger implication: The problem of sectional dominance was no longer one-sided but reciprocal. Reciprocal vulnerability drove both sides to resolve the problem (paralleling Rustow's 1970 observations). The Missouri Compromise did so through three components. First, it brought in Maine as a free state, balancing Missouri. Second, it divided the remaining U.S. territory between free and slave. Third, it made the balance rule explicit. For the next thirty years, states entered the Union as pairs (Meinig 1992, Potter 1976).

Balance and sectional veto had a profound effect on political behavior. First, the veto allowed each section to protect itself from onerous measures. Neither section, for example, could grow at the other's expense. Second, over the long run, balance provided the basis for sectional cooperation. Because it meant that radical measures could not succeed, balance induced moderates in each section to cooperate with one another. Moderates might have supported a sectional measure if it could have succeeded. The knowledge that it would fail, however, implying sure costs without any compensating benefits, helped turn them away from such temptations.

Pacts are not just agreements articulating valued ends. To attain these ends, pacts must provide for their enforcement. My discussion suggests the tools needed to analyze how they do so. It also shows how attention to enforcement helped bring peace in El Salvador and, following the Missouri Compromise, allowed democracy to survive in the United States for another generation.

The Consolidation of Democracy

A final topic is the distinction between the initiation and the consolidation of democracy (O'Donnell and Schmitter 1986; see also Burton, Gunther, and Higley 1992 and Diamond 1994a, 1996). The initiation or "instauration" (O'Donnell and Schmitter 1986) of democracy reflects the onset of its procedural aspects, such as regular elections. In addition to procedures, the consolidation of democracy requires that

> politically significant groups accept established political institutions and adhere to democratic rules of the game.... [D]emocratic consolidation is a "process through which democratic forms come to be valued in themselves, even against adverse substantive outcomes" (Burton, Gunther, and Higley 1992, 4, quoting Bolivar Lamounier).

In contrast:

> The dynamics of political conflict in unconsolidated democratic regimes are qualitatively different [from the dynamics in consolidated ones]. Important and powerful elites deny the legitimacy of the existing regime, and they seek to overthrow it.... [B]ecause they also perceive rival political parties as conditional in their support for democracy and equivocal in their commitment to democratic rules of the game, political competition and conflict are fraught with suspicion and distrust (Burton, Gunther, and Higley 1992, 31).

The study of democratic consolidation raises a paradox. Although consolidation requires a focus beyond democratic procedures, it occurs only when a society adheres to these procedures. A complete theory of consolidation therefore requires an understanding of what makes democratic procedures self-enforcing. As I have argued about other literature, studies of consolidation also have paid too little attention to the problem of enforcement. The literature characterizes many central differences between consolidated and unconsolidated democracies, but this characterization does not explain the differences.

Burton, Gunther, and Higley's characterization of unconsolidated regimes stresses that the party in power does not adhere to democratic rules in part because it perceives rival political parties as "equivocal in their commitment to the democratic rules" (1992, 31). The above discussion of Przeworski's model of democratic compliance shows how this perception makes it far less likely that incumbents will adhere to the rules. That discussion also reveals something missing from the elite-centric view in the literature, namely, the role of citizens and mass behavior.[35] Burton, Gunther, and Higley's characterization of unconsolidated regimes implies an absence of sanctions on political officials for violating the rules. If citizens were willing to punish officials—of all political stripes—for violating democratic procedures, then rival parties would be less likely to violate those procedures and hence, by the formula for compliance, so too would incumbents.

In terms of my approach, consolidation of democracy requires that a society resolve its social coordination dilemmas concerning the legitimate boundaries of the state. Resolving these dilemmas implies both that citizens will punish political officials who fail to adhere to democratic rules and that they will (indirectly) punish other citizens who fail to do so.

Many of the behavioral differences between consolidated and unconsolidated democracies directly parallel the model's two types of equilibria. My model suggests that elites and citizens in consolidated democracies have resolved their coordination dilemmas and play the Pareto-optimal equilibrium. This implies the existence of widely valued procedural and substantive limits on government, a citizenry willing to defend those limits, and the expectation among rival political parties that each will adhere to the democratic rules. These phenomena go hand in hand because they are different aspects of a single equilibrium process. A society, therefore, can sustain universalistic limits on the state. Political violence, intimidation, and repression are rare, and thus consolidated regimes are characterized by mutual trust and security (Dahl 1971).

In contrast, elites and citizens in unconsolidated democracies have failed to resolve their coordination dilemmas, so they play the asymmetric equilibrium in which universalistic limits on the state cannot be sustained. This implies substantial disagreements among the citizenry about procedural and substantive limits on the state, a citizenry unwilling to defend limits on government, and the expectation among rival political parties that each is at best incompletely committed to democratic rules. Violence and intimidation may be commonplace, and major political parties may advocate the overthrow of the government. There are no guarantees of respect for limits on governmental action. Mutual mistrust is the consequence.[36]

The model also suggests why consolidating democracy is so difficult. An unconsolidated regime, qua asymmetric equilibrium, is stable, implying that its combination of expectations and outcomes is difficult to dislodge. Although pacts are one route toward resolving a society's coordination dilemmas, if they do so incompletely or if they fail to provide for the enforcement of their provisions, then they will not move the society from the asymmetric equilibrium to the Pareto-optimal equilibrium of consolidation.

CONCLUSION

This paper develops a unified approach to the political foundations of limited government, democracy, and the rule of law—phenomena requiring that political officials respect limits on their own behavior. Those who lose elections must step down; they must follow established procedures for policy choices rather than impose them arbitrarily; and they must honor a series of civil and substantive citizen rights. The survival of democracy and the rule of law requires that political officials have incentives to honor a range of limits on their behavior; in other words, these limits must be self-enforcing.

My approach suggests that enforcement is at the heart of several puzzles in the literature on democracy, three of which I address in this paper: the uncertain relationship between interests and values in democratic stability, the problematic nature of ethnic divisions for democratic stability, and the role of elite pacts.

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[35] As Schmitter (1992) suggests, consolidation requires that both citizens and elites accept democratic institutions.

[36] Consistent with this view, Karl (1994) argues that a major barrier to the consolidation of democracy in Latin America is the inadequate civilian control over the military. This relegates many Latin American states to "hybrid regimes" that combine the nominal procedures of democracy with the potential for sudden authoritarianism.

In my approach, maintaining limits on the state is problematic because of a massive social coordination problem. Citizens can police the state only if they react in concert to violations of fundamental limits by withdrawing their support from the sovereign or political officials. The natural diversity of interests and experiences hinders the ability of citizens to react in concert; they are unlikely to have similar views about the appropriate limits on state action and the rights of citizens. Problems arising from disagreement are exacerbated because violations of the rights of one group often benefit another group.

The model implies that, in the absence of a consensus about the boundaries of the state, a coalition between the sovereign and one group of citizens against another is stable. This pattern is the most likely equilibrium and prevents society from maintaining universalistic limits on the state, and hence from sustaining the rule of law.

In the face of this problem, the successful transition to stable democracy requires the construction of a coordination device that specifies widely accepted and unambiguous limits on the state. By allowing citizens to react to violations in concert, such a device makes limits on political officials self-enforcing. Two means of constructing limits are elite pacts and the writing of a constitution.

Unfortunately, a society cannot establish a coordination device at just any time. When the state and its supporters benefit from transgressions against other citizens, this pattern is a stable equilibrium. Breaking this equilibrium is difficult and requires something exogenous to the model. A crisis, for example, may destroy the status quo.[37] Or economic changes—such as growth in the gains from establishing rights for the current victims of transgressions—may facilitate a bargain that enables the transgressors to discontinue old patterns without a fall in welfare. Finally, the economic and demographic growth of one group may allow a previously subjugated group to capture the state and impose new rules.

Central to my approach are the citizens who stand to benefit from potential violations of the limits on government. Maintaining limits on the state requires that citizens oppose a violation even if they potentially benefit from it. In a society that has resolved its coordination dilemmas, citizens oppose violations for the same reason players of the prisoner's dilemma forgo defection: Although it is costly today, they benefit over the long run. In states characterized by limited government, the intended beneficiaries react against violations (as the failure of Roosevelt's court-packing scheme illustrates); in states that fail to maintain limited government, intended beneficiaries sometimes support violations (as illustrated by the relative frequency of constitutional violations and coups in South America).[38] My model suggests that citizens in stable democracies not only must value democracy but also must be willing to take costly action to defend democratic institutions against potential violations.

My answers to the three puzzles raised in the introduction are summarized as follows. With respect to the relationship of interests and values: Although scholars have tended to focus exclusively on one or the other, my approach suggests that both are necessary to understand democratic stability. It is elites who choose whether to construct pacts, initiate democratization, violate citizen rights, and implement public policies. Mass behavior is relevant to elite choices because it determines part of elite incentives. In a society that has resolved its coordination problems, citizens hold the power to threaten political elites with loss of power if they violate agreed limits on government. When citizens have failed to resolve their coordination problems, however, some violations of citizen rights will go unpunished. Citizen values and elite interests are thus complementary aspects of democratic stability. Studies focusing exclusively on one or the other may illuminate important aspects of democratic stability, but they cannot tell the entire story.

The second puzzle concerns the problematic nature of deep ethnic divisions for democratic stability. Members of different groups often differ profoundly on the role of the state, the rights of citizens, and the appropriate ends of policy. In terms of my model, ethnic divisions raise barriers to resolving the social coordination problem. Divided but stable societies, typically in the developed West, maintain ethnic peace in part through the construction of a focal solution that resolves the society's coordination dilemmas. I illustrated this point with an interpretation of Lijphart's (1968) "politics of accommodation."

The third puzzle concerns the nature of elite pacts. My model suggests that successful pacts resolve the social coordination problem and hence make democratic limits on government self-enforcing for political officials. The literature has neglected this central aspect of pacts. My approach predicts that successful pacts will provide not only for the transition to democracy but also for their own enforcement.

I illustrated my claims about self-enforcing agreements and the complementarity between formal institutions and informal behavior by analyses of the recent peace accords in El Salvador (Wood 1995) and the Missouri Compromise in the United States (Weingast 1997a), as well as by Lijphart's (1968) "politics of accommodation." I also argue that the pact constructed during the Glorious Revolution created a focal solution to the coordination problems facing English elites. By permitting elites to act in concert, the pact raised the penalties for royal transgressions, making it in the interests of the sovereign to honor the pact. In other words, the new limits on the crown became self-enforcing.

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[37] Scholars have long noted dramatic institutional change following crises. Schumpeter (1991) emphasized the role of financial crises in the evolution of representative systems (see also North 1981, chap. 11). See also Burton, Gunther, and Higley 1992, Haggard and Kaufman 1995, and Moore 1966.

[38] Many examples can be given. Consider the first postindependence election that threatened government turnover in the United States (1800) and in Mexico (1828). In both cases, fundamental interests of incumbents and their constituents were threatened; in both, incumbents used political power to harass their rivals. Yet, their behavior differed when they lost. Federalists in the United States originated the "revolution of 1800" by allowing their arch-rival, Jefferson, to take power. In contrast, Mexican liberals compromised their constitution in 1828, preventing their rivals from taking office.

In a seeming departure from his earlier work, Przeworski (1995) challenges the approach taken here by raising another aspect of values, one consistent with Putnam (1993) and Almond and Verba (1963). He argues that democracy is not sustained merely through a self-enforcing equilibrium. Instead, democratic stability arises when democracy is venerated by citizens. Veneration implies a set of norms by which citizens automatically police potential violations. Instead, my model of self-enforcing equilibrium roots democratic stability in rational calculation: Citizens aid those who are threatened because the potential victims will later fail to come to their aid if they fail to come to the victims' aid.

My approach suggests that Przeworski is correct in a limited sense but wrong in a more fundamental one. In the limited sense, long-term stable democratic states appear to reflect widely shared and deeply felt normative structures. In these states, norms generated by veneration may well underpin citizen behavior that protects democracy, such as automatically rising against violations of political rights or democratic rules.

I claim that democratic stability rarely arises from veneration, however, because veneration is not antecedent to democracy's consolidation but is a product of it. In Rustow's (1970) terms, veneration evolves during the "habituation phase" of democracy, not the transition to democracy. The transition often involves elites who do not trust one another: for example, during a transition negotiated with an authoritarian regime, or one negotiated among previously warring elites. Establishing democratic stability in these settings requires that democracy be self-enforcing in the sense studied here. Przeworski's (1995) discussion of democracy in Spain, whereby citizens over time came to value democracy for its own sake, illustrates this point. Without democracy's initial self-enforcing properties, veneration would have no chance to evolve.

A range of topics remain outside the model. For example, it ignores the problem of how democracies ensure adequate governance. The approach also assumes away issues concerning the internal structure of groups, their collective action problems, and the effects of cross-cutting cleavages.

Finally, a number of extensions of this perspective are possible. First, although the model assumes only two groups, the results should generalize to any number under the appropriate conditions of symmetry among groups.[39] Second, various types of asymmetries among groups are possible. Only one group may hold the power to depose the sovereign by withdrawing support; for example, if the other suffers from collective action problems. Because it lacks the ability to punish the other group for failing to come to its aid, the weaker group is likely to be taken advantage of. Another asymmetry is that one group may be far more valuable as a coalition partner to the sovereign than the other, making that coalition more likely than the other. Third, we may derive a series of comparative statics results about what happens to particular equilibria as exogenous shifts occur, such as in the relative size and power of various groups, or changes in payoffs.

One of the central features of limited government is the rule of law, a society of universalist laws, not of discretionary political power. The above model provides a new approach to the microfoundations of the rule of law. Because laws and political limits can be disobeyed or ignored, something beyond laws is necessary to prevent violations. To survive, the rule of law requires that limits on political officials be self-enforcing. As we have seen, self-enforcement of limits depends on the complementary combinations of attitudes and reactions of citizens as well as institutional restrictions.

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[39] Symmetry here refers to the ability of each group to participate with others in deposing the sovereign. In the two-group model, each can depose the sovereign by joining with the other. The role of each is symmetric.

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The Rule of Wisdom and the Rule of Law in Plato's *Statesman*

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Recently, the idea of phronesis *has assumed great importance among political theorists. This prominence is due to the perception that, contrary to the application of the methods of natural science to politics,* phronesis *preserves the distinctive character of politics in its refusal to apply inappropriate standards of precision and to find certainty where it does not exist. But precisely because of this, the following question arises: Can we ascertain a nonarbitrary standard of* phronesis *without denying those characteristics that make it appropriate for the understanding of politics? I address this question by considering the treatment of* phronesis *(wisdom) found in Plato's* Statesman *and, in particular, the passage that weighs the relative merits of rule by wisdom and rule by law. In this seminal consideration of* phronesis *Plato (unlike Aristotle) does not confine his treatment to the practical sphere; even this intensely political passage contains far-reaching reflections on the nature of humanity and the world we inhabit. These explain what we are and what our world is, such that there is a need for this cognitive capacity. As I argue, they express the conditions of* phronesis *that can provide a guide for its use without negating its essential character.*

THE ISSUE

Recently, the idea of *phronesis* has assumed great importance among political theorists. This prominence is due to the perception that the application of the methods of natural science to politics neglects and thus distorts the irreducibly complex character of the political sphere.[1] But *phronesis*, the capacity for making sound judgments in varying circumstances, must be keenly aware of that which is particular, contingent, and fluctuating. As such, it preserves the distinctive character of politics in its refusal to apply inappropriate standards of precision and to find certainty where it does not exist. Yet, exactly because of this acceptance of imprecision and uncertainty the question arises whether any standard guides the use of *phronesis*. The available alternatives both seem problematic. An appeal to nature as a standard seems reductive, apparently denying the very openness that makes *phronesis* appropriate for understanding politics. Consequently, political theorists have looked instead to convention as a standard, whether understood as tradition, "shared human beliefs," or "the particular (and contingent) situation of men" (Beiner 1983, 92; Nussbaum 1986, 349). But this appeal leaves important questions unanswered: What are we to do when our conventions or traditions present us with conflicting norms, or when the very validity of these norms is challenged, or at the moment of founding when norms are not yet in place?[2] Our desire to apply *phronesis* to concerns beyond local or secondary ones moves us to seek a nonconventional guide for its use. The question, then, is can we ascertain such a guide for *phronesis* without denying those characteristics that make it appropriate for the understanding of politics?

I want to address this question through an examination of that passage in Plato's *Statesman* that weighs the relative merits of rule by wisdom (*phronesis*) and rule by law.[3] While it is not unusual to look to classical thought for guidance concerning *phronesis*, the usual authority is Aristotle rather than Plato. In fact, Aristotle is often portrayed as formulating the idea of *phronesis*, or "practical wisdom" as it is usually translated in the Aristotelian context, precisely in reaction to the hyperrationalistic abstractness of Plato's treatment of human affairs. Yet, this characterization cannot be accurate because in the aforementioned passage of the *Statesman* Plato himself investigates the distinctive character of *phronesis* or "wisdom" as it is often translated in the Platonic context.[4] More important, with respect to the issue under consideration, Plato's treatment may in fact be superior to Aristotle's. For what attracts contemporary theorists to Aristotle's practical philosophy is, in part, its apparent independence with respect to theoretical foundations, an independence which comports well with a reluctance to make theoretical commitments.[5] As the differing trans-

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[1] For helpful overviews of this perception, see Richard J. Bernstein (1985, 109–69, 207–23) on Gadamer and Arendt, and Paul Franco (1990, 120–40) on Oakeshott.

[2] On the pertinence of these questions as regards Gadamer's view, see Bernstein (1985, 162)

[3] As should be clear from my treatment of the dialogue I share the interpretive views of those who take seriously the dramatic form of the dialogue. This interpretive principle makes picking out one passage of a dialogue difficult and dangerous but I have tried to minimize this difficulty by setting its context, and by keeping in mind its connections to other parts of the dialogue. Finally, I am not claiming to provide "the Platonic view of *phronesis*" on the basis of one passage of one dialogue but rather that the dialogue treats this complex notion in a way that is instructive for the contemporary debate.

[4] Although Beiner (1983, 4, 24; 1994, 690) and Nussbaum (1986, 218, 237–8, 290–1) recognize this point, both interpret Plato as the proponent of hyperrationalism. For the view that Plato's and Aristotle's views of the relationship between theory and practice are basically continuous, see (referring specifically to the *Statesman*) Gadamer (1986, 123, 131) and Salkever (1990, 151–2).

[5] For examples of this reluctance see Beiner (1994, 690), MacIntyre (1984, 162–3), Nussbaum (1986, 349–52). This charge is found in the work of authors who think the rehabilitation of Aristotle is misguided

lations of *phronesis* suggest, however, Plato does not confine his treatment to the practical sphere; even in this intensely political passage of the *Statesman* there are far-reaching reflections on the nature of humanity and the world we inhabit. These reflections explain what we are and what our world is such that there is a need for this cognitive capacity.[6] These conditions of *phronesis*, I will argue, may provide a guide for its use that is both nonreductive and nonconventional.[7]

THE CONTEXT AND CHARACTERS

The *Statesman* follows the *Theaetetus* and the *Sophist* as the culminating dialogue in a trilogy devoted to the question, what is knowledge?[8] In the *Statesman* Socrates listens to the philosophic Eleatic Stranger as he attempts to define the statesman in terms of the statesman's knowledge.[9] The Stranger's treatment of this political theme, often ranging well beyond the usual scope of politics, indicates that the general theme of the trilogy animates this attempt; the character of the statesman's knowledge bears on the character of knowledge in general.[10] The Stranger's search for the statesman employs initially an abstract method of philosophy called diaeresis which, without reference to any notion of good, begins from the standpoint of comprehensive knowledge and relies on reason alone rather than experience (but cf. 264b11–c9). This method of theoretical analysis proceeds through a series of bifurcations that are supposed to lead to the isolation of the unique kind, or *eidos*, under investigation, defining it with precision.[11] The Stranger had used this method in the *Sophist* with somewhat dubious success. In the *Statesman*, the method reappears but now with explicitly comical results, and it is abandoned just prior to our passage. Just as the Stranger was able in the *Sophist* to distance himself from his intellectual forefather, Parmenides, so he is able to come to recognize the limitations of this universal method of theoretical analysis.

This ability is less evident in young Socrates, the Stranger's main interlocutor, who is introduced in a conversation between Socrates and Theodorus at the beginning of the dialogue. Young Socrates, a student of Theodorus, the mathematician, differs from his elder namesake in at least one crucial respect: He lacks awareness of his need to know. His discipline has invested him with an unwarranted confidence that he can easily grasp complicated subjects, making him all too ready to agree with whatever the Stranger says (262a5).[12] In young Socrates' case, knowledge is an obstacle to philosophy. The Stranger's pedagogical aim is, on the one hand, to bring young Socrates to recognize his neediness, to see that his abstract, theoretical understanding cannot answer all questions, without, on the other hand, reducing him to skepticism. Given this aim, the Stranger's conversation with young

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(e.g., Habermas 1979, 201) as well as those who think that the rehabilitation of Aristotle's understanding is incomplete without Aristotle's view of nature as a whole (see Salkever 1990, 30–6).

[6] This passage, and indeed the *Statesman* as a whole, have until recently been relatively neglected by students of politics largely, I think, owing to the view expressed by Ernest Barker that the "*Politicus* is intended to be a metaphysical exercise in the art of differentiation rather than a political treatise" (1959, 166). (As I indicate in the text, this is exactly why I think the dialogue is useful with respect to the issue under investigation.) Recently, however, the *Statesman* has received a good deal of attention with the publication of *Reading the* Statesman (Rowe 1995), the proceedings of the third Symposium Platonicum, a new translation from Cambridge University Press, Joseph Cropsey's (1995) treatment of the dialogue in his new book on Plato, and a new book-length commentary by Stanley Rosen (1995). I will distinguish my work from these in the notes below. But in general I think that the relevant papers in *Reading the* Statesman (those of Weiss 1995, Lane 1995, and Gill 1995) do not fully appreciate the anti-utopianism of the *Statesman*. As for Rosen, I believe that he does not adequately appreciate the extent to which nature guides *phronesis*. On this point (with reference to Rosen's earlier work) see Cole (1991, 207n18). Cropsey's view of nature is more ambiguous. He depicts nature as at best indifferent to human concerns, and at worst inhospitable because unintelligible (1995, 119–21, 124, 131, 141–2). But he also speaks of survival as "the natural reward that signals the presence of true and authentic statesmanship" (136). To the extent that he emphasizes the former view, my argument diverges from his. My understanding of the dialogue as a whole is, however, deeply indebted to Cropsey's understanding.

[7] I agree with Rosen concerning "the impossibility of codifying *phronesis*" but not that "it is impossible . . . to give a *logos* . . . of *phronesis*" (1995, 171, 169). While it is not possible to lay down hard and fast rules for its application, one can describe the "structure of the application of wisdom" and this is the purpose of my paper (p. 169). Given that a major theme of Rosen's book is the distinction between *phronesis* and *techne* (art) (p. vii) I would have expected him to concede that such a *logos* (account) is possible.

[8] The trilogy is set during the initial stages of Socrates' prosecution by Athens and the Eleatic Stranger is portrayed as a philosophic judge (*Sophist* 216b4–5). Plato interweaves Athens' trial of Socrates with Socrates' philosophic trial. There is a good expression of this point in Miller (1980, 1–15).

[9] While there is wide agreement that the decisive criterion of statesmanship in the dialogue is the possession of some kind of knowledge there is controversy concerning its content. The interchangeable use of *episteme* (knowledge or science) and *techne* (e.g., 258b6) manifests the problematic status of this issue. In the passage under consideration wisdom too must be included in the statesman's understanding.

[10] On the relationship between Socrates and the Eleatic Stranger, see Cropsey (1995, 134, 140), Mara, (1981, 377–80), Miller (1980, 8–100), and Sayre (1969, 150–4). Cropsey suggests that the Eleatic Stranger is superior to Socrates. Sayre also sees the Stranger as superior in his possession of an analytic method that actually produces definitions in contrast to the merely refutative character of Socrates' approach. Mara accepts this view of the Eleatic and Socrates but finds the Socratic way superior. In my view the *Statesman* shows the Stranger's transcendence of this technical method and his adoption of the Socratic way of dialectic.

[11] For an excellent treatment of this method as used in the *Statesman*, see Miller (1980, 16–33). It should also be noted that the Stranger never actually refers to an *eidos* of the statesman nor does he refer all to Ideas as they are presented, for example, in the *Republic*, *Phaedrus*, or *Phaedo*. But cf. Skemp (1987, 74–7, 241–2) and Saunders (1992, 466–7, 485, n11). I have used "*eidos*" to designate the goal of diaeresis but in this dialogue *genos* (class) is used interchangeably with *eidos* (Miller, 1980, 125n200 and Griswold, 1989, 142–3).

[12] On the character of young Socrates, see Miller (1980, 5–8) and Benardete (1963, 207, 210). My understanding of the drama of the dialogue is much indebted to these authors though I differ from them in some specific instances. On the importance of young Socrates being a mathematician, see Benardete (1984, III.80, 940), Annas (1995, xiii), Hemmenway (1994, 254–6), and Miller (1980, 7–8). Skemp (1987, 26, 67) connects the appeal of diaeresis to young Socrates' mathematical training.

Socrates can also help us consider the basis of a knowledge that is imprecise but not arbitrary.

Prior to our passage this conversation has been littered with dead-ends and digressions. In the Stranger's initial attempt to define the statesman, the method of diaeresis promised more precision than it could deliver. It foundered particularly on the assumption that the world is divided into internally homogeneous kinds, or *eidê*. Left unexplained was the diversity within an *eidos*, most importantly within the human *eidos* whose definition the Stranger deems essential for understanding the statesman's political science. Specifically, the method could not account for the potentially vast difference between ruler and ruled and so led to the conclusion that humans are a homogeneous herd ruled by a shepherd belonging to a different kind.[13]

To correct this mistake the Stranger turns to a complex myth describing the governance of the cosmos. He relates that in one age, the age of Cronus, the god rules directly and so there is no scarcity, no conflict, and no politics; all that is required for life is provided spontaneously. However, our age, the age of Zeus, is characterized by harshness, scarcity, and conflict because the god does not rule. Rather, whatever order exists derives from the rule of other humans. By including the ruler within the human *eidos*, the myth maintains an underlying equality between ruler and ruled. But at the same time it indicates that the human *eidos* comprises significant differences.[14] The myth explicates the source of these differences in its reflection on human needs. The Stranger relates that in the age of Cronus bodily needs, needs that are shared by all, are fully satisfied. But he questions whether these sated beings experience *human* happiness, whether specifically human needs are satisfied. He makes the issue turn on a need shared by relatively few: the need to engage in philosophy. And he makes it doubtful that anything in their world would have confronted them with the difficult choices, the dilemmas and perplexities, which awaken the need for philosophy (272b10–d6).[15] He thereby suggests that the full satisfaction of one set of needs, bodily needs shared by all, obstructs the satisfaction of the other, those of the intellect which are fulfilled in relatively few.[16] In bequeathing to us these diverse needs nature fails to provide for spontaneous human wholeness. This disorderedness or diversity of human nature, amplified by the disorder of the world we inhabit, underlies the Stranger's forthcoming reflections on the rule of wisdom and the rule of law.[17]

With the mistake of the initial diaeresis corrected, the Stranger pursues the definition of the statesman by a new method, one that better reflects the complex human character which is the object of the statesman's concern. He quietly replaces the diaeretic method with a dialectical approach that relies not only on reason but also on intuition, experience, and opinion (287b10–c4).[18] This approach is evident above all in the passage on which I wish to focus. At the end of this passage the Stranger claims that he has successfully separated the political art from its sham competitors. But the road to that destination is anything but direct. As the passage unfolds, positions are stated, challenged, and modified in response to further arguments. There is no theoretical analysis leading to a precise answer; indeed, the discussion which follows is characterized by questionable analogies and sudden shifts in perspective. On the whole, it seems designed to train young Socrates, as well as the reader, to appreciate differences between similar things and to accept that some objects of investigation may comprise irreducible differences—in short, to become adept at the use of *phronesis*.[19] But more important, this dialectical judgment of the relative merits of the rule of *phronesis* and the rule of law precisely explicates the need for a capacity such as *phronesis*. What we will see is that the Stranger does trace that need to an understanding of human nature. But far from a reductive account, he understands human nature as uniquely diverse and variable. And he finds in this disorderedness the nonconventional but nonreductive guide for the proper use of *phronesis*.

YOUNG SOCRATES' PROBLEM: THE CHARACTER OF THE STATESMAN'S KNOWLEDGE (291a–294d)

The passage begins with the Stranger introducing the greatest competitors of the statesman, the run-of-the-mill political leaders whom he refers to as "the greatest imitators and greatest enchanters" and as "the sophists of sophists" (291c1, 303c3–5). The task is to separate these imitators from those "who are in their being statesmen" (291a8–b3).[20] Young Socrates responds eagerly to the Stranger's intention to see these impostors "as plain as day" (291c5). To this end, the Stranger begins to distinguish the statesman's knowledge from that of the politician-sophist. However, he does so indirectly through the introduction of a typology of regimes. More specifically, the Stranger adduces the usual view of politics, a view that places importance on such questions as whether rule is by force or consent, by the wealthy few or the many poor, with law or without. On the basis of these criteria he distinguishes kingship and tyranny, aristocracy and oligarchy, and democracy. But in response to young Socrates' acceptance of these criteria the Stranger reminds him of their earlier agreement that, in fact, the possession of political science alone is decisive in politics (292b1). It seems then that the Stranger introduces the usual view only to dismiss as irrelevant all nonepistemic criteria of politics, criteria which distinguish the variety of regimes. Yet, near the end of our passage he offers another typology of regimes based on these criteria, a typology in which democracy is divided into lawful and lawless, and this distinction is taken very seriously (303a2–b5). In the intervening discussion he suggests to young Socrates the *reasons* for the importance of these nonepistemic criteria. These reasons begin to emerge in what follows.

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[13] Weiss (1995, 217 n17) rightly notes that humans are referred to as a herd subsequent to the myth but concludes, wrongly in my view, that the herd image should not be taken prior to the myth to mean that humans are homogeneous. The postmyth references to the human herd serve to emphasize the vast difference within the human *eidos*. It is not established that this difference is comprised by the human *eidos* until after the myth. On this point, see Miller (1980, 31–2, 94–5).

[14] Weiss (1995, 216) captures the way in which the myth expresses both human equality *and* inequality.

[15] On this point, see Saxonhouse (1992, 127).

[16] Contrary to Weiss (1995, 216), this radical diversity of needs is the key point, not simply their multiplicity.

[17] It also helps explain why human affairs must be judged not only by a relative measure, one that presumes commensurability, but also by a measure understood as a standard, the mean (283b1–287b3).

[18] Miller (1980, 90–3, 102–3, 110–30, Griswold (1989, 141–20) and Gill (1995, 292) recognize the dialectical character of the passage. My account of this dialectical movement differs from these commentators regarding both its structure and its basis in the nature of the human things.

[19] The Stranger's purpose in discussing politics with young Socrates is not then to alter his political views (cf. Gill 1995, 292–3), but to instruct him concerning the character of knowledge, the most problematic aspect of which is human knowledge of the human.

[20] The translations, except where noted, are those of Benardete 1984. Most important, Benardete translates *phronesis* as "intelligence" but I think "wisdom" preserves the element of judgment in a way that "intelligence" does not. The Greek text is Burnet 1979.

Insisting that knowledge alone is decisive in politics, the Stranger reminds young Socrates of their earlier discussion which held that "royal rule is some one of the sciences" and, he adds, "we certainly chose and separated from the rest a certain *critical* and supervisory (science)" (292b6, 292b9–10 emphasis added). In fact, they had earlier agreed that political science is supervisory but not critical, more akin to architecture than accounting. Now he asserts it is both. But how can political science be both supervisory and critical, both practical-productive and theoretical (gnostic)?[21]

So far, the dialogue has offered evidence for both views. On the one hand, in the initial diaeresis the Stranger assigns political science to the gnostic arts, those which are "stripped of actions and furnish only cognition," rather than to the practical (258d5–6).[22] And it is repeatedly claimed that ruling requires science alone, all other qualifications being unimportant. On the other hand, there is reason to think that the method of diaeresis itself involves production, that we produce the *eidê* as we "stamp" them or by "putting the seal" on them (258c4–8).[23] Early in the dialogue the Stranger indicates that differing intentions can produce variant divisions of the subject matter (258b6–c8, 265a1–266e11).[24] Finally, the paradigm for statesmanship becomes the productive art of weaving.

Nevertheless, if all were a matter of production, there would be no intelligible distinction between the speech of the imitator and that of the statesman. The Stranger's forthcoming claim to have separated out the political art would be undermined. We must, then, take seriously the Stranger's claim that political science involves both critical and supervisory aspects. It is certainly difficult to be precise about a science that is somehow both theoretical and practical-productive (292c2–3).

For his part, young Socrates does not yet recognize the problem. It does not bother him that they are incapable of being sufficiently precise about what political science is, even though he is certain that its presence in the ruler is *the* criterion of the right regime (292d1). The Stranger asks a question whose implications could shake young Socrates' certainty: "Well, in a city of a thousand men is it possible for some hundred or even fifty to acquire it [the science of ruling human beings] adequately" (292e4–5)? Young Socrates responds with his longest speech in the entire dialogue, a speech that makes two general points (292e6–293a1). First, young Socrates is pleased to maintain that science is the possession of very few, that in this decisive respect humans are radically unequal. Second, he remembers their agreement that knowledge alone equips one to rule. But precisely if there is such a vast discrepancy between the one or few who possess and the many who lack this knowledge, how can knowledge alone suffice for rule?[25]

Rule, it seems, would have to involve other than simply epistemic means of implementation. The Stranger had alluded to this problem when, in introducing the wise king, he said that they must distinguish him from those "who pretend that they are statesmen and *persuade many* but are not in any way" (292d7, emphasis added). The diversity of humans with regard to intellectual capacity leaves room for error concerning the identity of the true statesman; demagogues succeed while the words of the true statesmen go unheeded. Young Socrates fails to appreciate the contradiction between the two positions, both of which he expresses so confidently. He does not see that the very thing on which he prides himself, his intellectual ability in relation to others, points to the limitation of the rule of knowledge alone.

This lack of understanding of the implications of human inequality or diversity can be traced to a characteristic shared by young Socrates' mathematical orientation and the diaeretic method used by the Stranger in the first part of the dialogue.[26] Both presume the homogeneity of their objects. Thus, both obscure the recognition that the *eidos* may refer not only to those individuals joined by their sharing in some common characteristic (by virtue of which they are distinguishable from others as a class) but also to that common characteristic itself in terms of an end which the individuals more or less perfectly embody.[27] Accordingly, young Socrates is hard-pressed to grasp the kind of being we are, a being that strives to fulfill a potential.[28] He fails to understand the significant difference reflected in his own estimation of the number of true statesmen. But in his failure to see this discrepancy between his preferred mode of knowledge and his assumptions concerning human beings, young Socrates' speech points to the problem with which we are concerned: What kind of knowledge is appropriate to beings exhibiting such diversity?

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[21] Previously, both supervisory and critical arts were placed among the gnostic (theoretical) arts (259d6–60b1), but this division itself suggests the problematic character of the distinction between theory and practice which is thematic throughout the *Statesman*. Rosen (1995, 150) interprets this point differently.

[22] But note here (260c6) the Stranger's characterization of *their* activity as observation (and distinct from the statesman's activity) which points to the ambiguous relationship between gnostics and practics.

[23] On the productive character of diaeresis, see Campbell (1988, 5 n7), Cropsey (1995, 113), and Rosen (1993, 61–6).

[24] As will become clear below, the ambiguous meaning of *eidos*, and especially the human *eidos*, is of central importance in the teaching of the *Statesman*. Useful considerations of this ambiguity can be found in Benardete (1963, 193–226) and Moravcsik (1973, 324–48).

[25] The Stranger previously made young Socrates' first point and joined it with the idea that political rule should not be distinguished from any other kind of rule (e.g., mastery, kingship) since rule is simply a form of knowledge (259a1–4). But young Socrates' other point suggests that Plato is aware of the distinctiveness of political rule (cf. Aristotle, *Politics* 1252a8–17).

[26] On the similarities between mathematics and *techne* see Roochnik (1990, 18–21).

The Stranger proceeds to draw a comparison between the science of the physician and that of the statesman which makes young Socrates feel at least some wariness regarding the unimpeded rule of the one with political science. He begins the analogy saying that the only thing we care about regarding physicians is that they cure us. Whether the physician is rich or poor, uses writings or does not, even whether we are willing or unwilling patients—all this is irrelevant as long as the good of health is achieved. In making this point the Stranger lists several treatments the physician might use including "surgery, cautery, or . . . the application of some different pain" as well as "purging or slimming (us) in a different way, or maybe increasing us" (293b2–3, 5–6). Likewise, the ruler might "purge the city for the good by killing some or maybe exiling" or "make the city smaller by sending out colonies" (293d4–6). It does not matter "as long as they are employing science and the just and, in keeping it safe, make it better from worse to the best of their ability" (293d8–9).

Through his reflections on the status of examples earlier in the dialogue as well as his revisions of earlier examples, the Stranger has taught us to consider his analogies carefully. The present analogy warrants such scrutiny. Unlike the case of the physician, it is not clear who or what is the "patient" of the ruler. Given the need to exile or even kill some for the sake of the city's good, there would seem to be a difference between this good and the good of the individuals who constitute the city. Moreover, the goal of the ruler is not only to keep the city safe but also to make it better. While the requirements of security may indeed involve the sacrifice of individuals, making the city better, to the extent that this is distinguishable from greater security, would involve a consideration of the good of the individuals who constitute the city. In sum, the goods at which politics aims, in their diversity and even incompatibility, are problematic in a way that the good of health is not. The Stranger recognizes the difference between the two when he refers to the true ruler as "employing science *and the just*" (293d8, emphasis added). This first mention of justice in the dialogue raises the possibility that the just falls outside of science, that the diversities among humans, and among human goods, cannot be reconciled so as to be pursued by an algorithmic method following precise steps to a single, clearly definable goal.

The Stranger concludes that the only legitimate regime is one in which the possessor of political science rules directly, but his portrayal of the ruler performing radical surgery on the body politic succeeds in provoking young Socrates to make an objection (293e2–3). He accepts all that has been said except for the proposal of direct rule, rule without law.[29] The Stranger's disanalogy has opened young Socrates to the possibility that in the realm of politics the applicability of knowledge is somewhat more complex than he had suspected. He uses this opening to exhibit to young Socrates why there is a need for the cognitive capacity, wisdom, the existence of which is a recognition of a gap between general understanding and concrete particulars. The character of wisdom emerges, appropriately, from the Stranger's critique of such a generalizing viewpoint—namely, that of law.

WISDOM FROM THE PERSPECTIVE OF LAW (294d–297c)

The Stranger states that while "in a certain sense the legislative (art) belongs to the royal (art), the best thing is not for the laws but for a man—the king with wisdom (*phronesis*)—to have strength" (293a6–8). In a far-reaching statement he presents the view of humankind underlying this claim:

> Because (a) law would never be capable of comprehending with precision for all simultaneously the best and the most just and enjoining the best, for the dissimilarities of human beings and of their actions and the fact that almost none of the human things is ever at rest do not allow any art whatsoever to declare in any case anything simple about all and over the entire time (294a10–b6).

Human beings are so dissimilar or diverse, both in themselves and in their actions, that no general principle, at least no general and *simple* principle, can comprehend them. Adding to their elusiveness is their great variability so that they are *almost* never at rest. Left open is the possibility that a more complex understanding could discern some locus of fixity amid this variability. But unity and fixity must not be imposed where they do not exist. And it is clear that the expressed character of "the human things"—human beings and their affairs—makes dubious a direct application of this knowledge of humankind to specific human activities and individuals.

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[27] On this point, see Dorter (1987, 112). Socrates' doubts about the possibility of measuring the relative worth of the philosopher, statesman, and sophist, expressed in reaction to the mathematician Theodorus, suggest the limited usefulness of mathematics in understanding differences among humans (257a1–b7).

[28] On this point, see Benardete (1963, 198–202). Cf. Rosen (1995, 162) who states that there is no philosophical doctrine of human nature in the *Statesman*.

[29] Skemp (1987, 47 and n3) maintains that young Socrates finds rule without law objectionable because for him, like any Greek, it sounds too much like tyrannical dictatorship. Agreeing with Skemp are Gill (1995, 293) and Lane (1995, 284). Strauss (1975, 218) suggests rather that his reaction follows from the mathematician's insistence that knowledge must involve the ascertainment of regularities, of laws, whether in politics or in nature generally. Yet the Stranger had mentioned rule of the wise previously without objection from young Socrates. Perhaps the example induced young Socrates to apply to politics his penchant for generalities.

The error of law lies precisely in its pretension of providing a principle applicable with precision to each and all. In its overly general and inflexible character—shared with the diaeretic method—law thus mistakes the character of the human things. More apt is the cognitive capacity that can take account of difference and alteration and that does not presume the perfect subsumption of individuals within some general category in the manner of the mathematical understanding familiar to young Socrates. Thus, in comparison with the rule exercised by the one with wisdom, rule by law is like "some self-willing and foolish human being who allows no one to do anything contrary to his own order or even for anyone to ask a question" even if some other way is clearly superior (294c1–4).[30]

Given this criticism it is surprising when the Stranger adds: "On account of what is it necessary to legislate, inasmuch as the law's not most correct? One has to discover the cause of this" (294c10–d1). In the ensuing investigation the Stranger discovers the cause that underlies this significant judgment, that that which is "most correct," the rule of wisdom, must be qualified by that which is "necessary," the rule of law. He thereby exhibits the way in which the foregoing understanding of human being guides the exercise of wisdom.

The Stranger begins with an example drawn from gymnastics which indicates that while the feasibility of general rules lies in the requirement that many be trained together, the necessity for such rules stems from the comparative paucity of those fit to be trainers (294d10–e2).[31] The Stranger proceeds to compare the trainer with the legislator, "who is to supervise the herds about the just and their mutual contracts" (294d8–295a1), politics being considered here as an association for mutual exchange focused on the needs of the body which all share. Again, this requirement for common activity makes feasible the legislator's supervision of humans in groups. In this supervision, the legislator uses not only laws but "ancestral usages" to lay down rules "for the many and the most part" (295a6–7, a4). The reference to ancestral usages, which are obeyed in an habitual manner, makes especially clear the difference between these coarse approximations and direct rule by wisdom alone. But as the locution of herd and legislator suggests, the necessity for such general supervision seems again to lie in the relative paucity of the capacity to rule; there are too few of the wise in relation to the many who require direction.

Yet, now the Stranger makes a summary statement which points beyond the foregoing practical difficulty. He leaves it as hypothetical whether even the one with the royal science would be capable of ruling all with precision. Nor does he say that this one would *never* use laws (295b2–5). He implies that the incapacity to be both comprehensive and precise is more than a practical difficulty because it is not clear that they can be perfectly reconciled even by the wise.[32] The Stranger's allusion to military training points to the difficulty. In speaking of gymnastics, he mentioned drill instructors who assign equal toils to men in groups which all can remember since it is engaged in "among you all as well as elsewhere in different cities" (294d3–e6). Such a regimen is needed to ensure cohesive group activity because in war what is suitable for each, to remain alive, may not be reconcilable with the collective good of victory. Law, and even better, ancestral usages, may be necessary to ensure discipline when these goods cannot be rationally reconciled. The question is whether this reconciliation is ever more than an approximation.

Young Socrates hesitatingly agrees with the Stranger's statement and the Stranger responds that it is "still more" the case on the basis of what he is about to say (295b7–8). But, given the ambiguity of his preceding statement, it is difficult to know what is to be "still more" the case. The question under consideration is, why it is necessary to legislate? but the analogy which follows seems to illustrate the inadequacy of law. In light of this perplexing exchange we should consider the next analogy with care.

The Stranger tells of a physician who, going abroad, leaves behind written instructions for his patients. He deems it laughable if on his return the physician felt himself still bound to his earlier prescriptions even though conditions had changed. When something "occurs contrary to expectation (one of those things from Zeus that are somehow other than the usual)," the physician reasonably adjusts his view of what is good for the patient (295d1–2). It would be ludicrous for him to think that he must not "trespass the ancient things that were once legislated" (295d3). "Science . . . and true art" would welcome all such adjustments as progress (295d7–e1). It would thus be laughable to fetter the physician because this would clearly interfere with the universally agreed-upon goal of medicine, health.

But the Stranger's completion of the analogy poses this question: Is the refusal to trespass the ancient things in the face of unforeseen conditions as laughable in the case of politics? He asks:

> But whoever wrote, and legislated without writing, the just and unjust, noble and shameful, good and bad things for the herds of human beings—all that are grazed city by city in conformity with the laws of the onetime writers—if he who wrote with art, or someone other similar to him,

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[30] That this image, used to critique law, should also make us wary of rule by an individual exemplifies the Stranger's constant shifting of perspective within the broader shifting of the argument (Campbell, 1988, 138 n8).

[31] The Stranger aloofly asks a question about the existence of gymnastics while sitting in a palaestra. For Campbell (1988, 138–9 n22) this exemplifies the later Plato's lack of interest in drama, but this aloofness is indeed dramatic and helps us to understand the orientation of the Eleatic Stranger.

[32] The two arts of measurement also express the distinction between the precise and the comprehensive.

> comes back, shall he really not be allowed to order other things contrary to these? Or would this prohibition too no less than the former appear in truth laughable (295e4–296a2)?[33]

As the motif of "going abroad" suggests, the Stranger speaks here of the founder rather than the ruler.[34] The difference between the founder and the community exceeds that between rulers and ruled because the decisions of the founder govern not only his contemporaries but shape lives long after his death. Moreover, the founder as creator of the political community is more acutely aware of the variability of human possibilities than is a ruler working within a system. Accordingly, as I argue in what follows, the Stranger leads young Socrates to reflect on founding, not to launch his political career but because this fundamental political act provides a window through which he might comprehend the uniquely diverse and variable character of the human things.

Reference to the human herd, prominent in the early portion of the dialogue, had been dropped after the myth. Its return signals that the difference between the founder and the citizens of the community may not equal that between shepherd and herd, but it is nevertheless vast.[35] In light of this difference, the goal of the founder becomes more complex than that of the physician because it is not at all clear that his concerns—the just, noble, and good—are perfectly compatible with one another. The Stranger's addition of the noble to the previously mentioned goals of the good and the just is itself a recognition of the inherent controversy among the human goals and among the humans oriented on them. For the noble—for example, the sacrifice of oneself for another—is not reducible to either the just or the good. While the community may well require such noble acts for its preservation, the noble act goes beyond the requirements of justice, beyond what can legitimately be *expected* of citizens. But neither can it be clear, without some transhuman support, that such sacrifice is good for the individual who performs this act. The very existence of the noble confirms the tension between goodness and justice, a tension present in the act of founding itself; if the founder must go away from the community for the sake of his creation, then this act cannot benefit the founder in the same manner as it benefits those who remain in the community.

Founding is necessary because in the age of Zeus the god does not provide order. Founding is possible because of the human possession of intelligence. This dependence on human intelligence magnifies the importance of diversity among humans. It magnifies that diversity within the human *eidos* between that which all share and that which characterizes some fewer excellent individuals. It magnifies those differences that reflect the diverse needs articulated in the great myth of the reversed cosmos. Those few who create a way of life for others do almost stand in relation to them as shepherd to herd or as god to humans.[36] The act of founding thus illustrates that unique diversity of human goods substantiating the Stranger's claim that a "law would never be capable of comprehending with precision for all simultaneously the best and the most just" (294a10–b1).

The human provision of order illustrates as well the unique variability of the human things.[37] In legislating that which concerns the just, noble, and good, the founder exercises a discretion regarding the goals of his art that is not available to the doctor regarding health. The founder molds human beings to a remarkable degree if, in a significant sense, what we are is a function of our beliefs concerning the just, noble, and good. While all that is corporeal is subject to unpredictable change or chance, it is more characteristic of human things. For the diversity of human needs makes the character of human wholeness itself a question and so makes the very character of the human things a matter open to variety, controversy, and potentially arbitrary decision. Testifying to this fact is the enduring variety of political communities whereby humans are "grazed city by city," the variety that is in part responsible for all cities preparing for war (295e6–7). In sum, the extraordinary latitude regarding what it means to be human, open to every human but seen most vividly in founding, opens the door for arbitrary decision and thus unique variability regarding the character of the human things themselves. Hence, the Stranger's claim that "almost none of the human things is ever at rest" (294b3–4).

The necessity and possibility of politics illustrates then the unique diversity and variability of the human things. Causing this diversity and variability is what might be called the disorderedness of human nature, the different and even irreducible needs which make the wholeness of the human *eidos* a question. The need for humans to provide an answer to this question, that is, the uncertainty of any supervening order in the age of Zeus, only serves to amplify this disorder.

In illustrating this point, the disanalogy of the physician and the legislator would seem to illustrate precisely the way in which the human things elude any fixed law. But these reflections occur in a demonstration of why it is "necessary to legislate" (294c10). Why then does the Stranger assume that the best founder, presumably the possessor of wisdom, will use laws (294a6–7, 295e4–8)? The answer is that while the nature of the human things resists the fixed generalizations of law, so great is this diversity and variability that it is the dictate of wisdom itself that it order human life by the imposition of law, as if rule-governed understanding of the human things were possible.

In the face of the variability of the human things, wisdom points to law as a necessary imposition of a bulwark against ceaseless and arbitrary change, the things "contrary to expectation" (295d1–2). The endurance of "writings" provides the possibility of meeting the variability of human things by preserving wisdom in however diluted a form beyond the demise of the wise founder, when the founder "goes abroad" permanently (295c1). This point is also important as regards human diversity, for the source of our concern about arbitrary change lies in the differences among us with respect to intelligence. Given such differences, the founder's true intention may remain opaque to the many. Moreover, how will we know that the one who wants to rewrite the law is in fact "similar to him" who wrote it initially (295e8)?[38] In the face of this diversity wisdom again points to the rule of law which, in its impersonality, provides the possibility of ameliorating a conflict that otherwise must appear to those involved as the imposition of one's interest on another.

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[33] I have altered Benardete's translation from "beautiful" to "noble" in order to bring out the political bearing of the speech.

[34] This is recognized by both Campbell (1988, 141n9) and Lane (1995, 283). Miller sees the shepherd-herd metaphor as pointing to the vast difference between "the one who first . . . establishes the *nomos*" (law) and the many (94–5). See also Waterfield's translation of this passage (Annas, 1995, 61).

[35] See Miller (1980, 94–5) and Benardete (1984, III.102).

[36] For a contemporary recognition of this point, see Steinberger (1993, 304).

[37] On this point, see Lane (1995, 284).

The nature of the human things themselves determines that the ultimate act of politics will involve the imprecision of law and of "ancestral usages" which have their effect through habit and that the founder may even "go abroad" to give himself and his creation the color of divinity. And these devices are "still more" necessary when politics considers not only the regulation of mutual exchange but also the formation of human character for, in pursuing this latter goal, it must rely less on the powerful motive of self-interest (295b7).[39] It is perhaps, in light of this necessary imprecision, that in his first mention of justice the Stranger distinguishes it from science; there is a precision and certainty regarding the objects of science that is not available regarding justice (293d8). Accordingly, that which may be laughable in the case of medicine, the reliance on law and on the habitual and the ancestral, is less laughable in the case of politics.[40]

Young Socrates does not yet grasp this character of politics, thinking that the founder's reluctance to change the laws would be as laughable as in the case of the physician. In one further attempt to convey to him the complexity of the human *eidos*, the Stranger reports what the many say "in a case of this sort" (296a4)—namely, that the new legislator may "legislate once he's persuaded his own city, but not on any different condition" (296a8–9). The Stranger returns to the example of the physician, offering what seems to be a counterexample to the view of the many. But that view is necessarily present as he focuses on this problem of communication opened up by the difference between ruler and ruled. The Stranger indicates that, counter to the explicit thrust of his analogy, there may be reason to distinguish the physician's use of force for our health from that of the statesman for political goals. He concedes that there is "blame attached" to force of the latter sort (296d1).[41] To reject this force might be "ridiculous," but we have already seen that that which is ridiculous in medicine may not be so in politics (296d2). Human diversity is such that even the true statesman might have to force people to do that which is truly good for them. From the standpoint of the people, however, the activity of the true founder may also (and perhaps necessarily) appear blameworthy. It is this diversity which renders the nonepistemic criteria of politics—the distinctions between force and persuasion, coercion and consent—politically important. It is this diversity which produces the tension among the goals of politics to which the Stranger now calls attention:

> And there is no mistake in everything intelligent rulers do, as long as they maintain one big thing—as long as they always distribute to *those in the city* that which with mind and art is most just, and can keep them safe and make them better from worse as far as possible (297a5–b3, emphasis added).

Earlier, he had stated that the goal of the statesman was the good of the city, but now it is the advantage of the ruled. Furthermore, what the Stranger refers to as "one big thing" is in fact a collection of goods whose requirements are in tension. He reiterates the source of this situation when responding to young Socrates' conclusion that one cannot "speak in contradiction" to the rule of the wise, he adds that "one must not speak in contradiction" either of the fact that "a multitude of no matter whom would never be able to get the science of this sort and manage a city with mind" (297b7–c2). The difficulty remains that expressed unwittingly by young Socrates' in his longest speech: the problematic character of our knowledge in the face of an object comprising such diversity.

The purpose of the Stranger's conversation with young Socrates is to enable him to appreciate this difficulty and its implications for the character of knowledge. The Stranger does not intend simply to induce young Socrates to accept the unlimited rule of wisdom. Rather, his alternation of perspective, his dialectical procedure, aims to show young Socrates the partial truths expressed in the imperfectly reconcilable positions of the wise and the many. More important, he focuses on this political problem as the most vivid representation of the complex human *eidos* underlying these divergent claims. Were young Socrates to recognize this complexity he would gain insight into the inadequacy of a method such as diaeresis. He would see that what is required instead is a cognitive capacity that is open to the differences, the variability, the enduring complexity comprised by such an *eidos* as the human. For it is *phronesis*, understood as wise judgment, which attempts to consider the particular in the light of the universal. Accordingly, it is keenly aware of the irreducible difference between the two as expressed here in the enduring tension between what we as humans all share and those individuals who more fully embody what it means to be human.

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[38] See Lincoln's "Lyceum Address" on the difficulty of recognizing the motives of the re-founder.

[39] See *The Federalist Papers* 49 for recognition that these considerations are still relevant even in a community that relies heavily on the motive of self-interest.

[40] On this point, see Annas (1995, xvii).

[41] While the Stranger speaks of physicians who use force, he calls those who use force in politics, enforcers. He asks the name of this force but then prevents young Socrates from answering (296b1–3). Earlier in the dialogue they had decided that the name given to such rule is tyranny (276e6–13).

For the present purpose, however, it is especially important to see that the need for *phronesis* rests on a view of the *nature* of the human things as *diverse* and *variable.* By understanding the conditions of *phronesis* in this way the Stranger has pointed to the possibility of a guide for its use that is *nonconventional* as well as *nonreductive.* He has exemplified the workings of this guide in his judgment that *phronesis* must limit itself in the rule of law. As he will continue to show in what follows, the Stranger's conversation with young Socrates leads us to see that recognition of the inappropriateness of the generality and certainty presumed by a method such as diaeresis need not consign us to arbitrariness if our judgment can be guided, however indirectly, by the nature of the human things themselves.

LAW FROM THE PERSPECTIVE OF WISDOM (297c–303c)

Young Socrates' dialectical education continues as the Stranger looks at law from the perspective of wisdom. More specifically, having seen why the best (the direct rule of wisdom) should be qualified by the necessary (the rule of law), we now see why, and in what way, the necessary should be informed by the best. The focus will be on regimes other than the one right regime, regimes which the Stranger refers to as imitations of the best. This reference elicits young Socrates' uncertainty regarding imitations. The idea of imitation, of something approximating something else, is one which eludes his understanding in the same way that the idea of potential proved elusive. Understanding both depends on grasping that a thing can be considered as it is in itself but also as it is in relation to a standard. This proves to have a political bearing. For as the Stranger proceeds to explain imitations, he shows that "the mistake that now occurs" about them lies in the failure to recognize the difference of the one best and the many lesser regimes (297c8–d1).

He begins the discussion with the extreme claim that imitative regimes will punish with death anyone who speaks or acts contrary to the laws (297e2–3). To clarify the point the Stranger returns to the images of the captain and the physician. In this discussion he articulates the many's fear and distrust of the few which underlies several of the political institutions familiar to us as liberal—not only the rule of law but also popular rule, election, and a nascent constitutionalism.[42]

Speaking for the ruled, the Stranger tells of a physician and a captain using their arts, malevolently and treacherously, in pursuit of personal gain. In response to this corruption of the arts, he lays out the plan to oversee these and all other arts by "an assembly of ourselves" comprising both the wise and the unwise (298b7). Furthermore, "what the multitude resolve about these . . . we write on some kind of *kurbeis* [wooden pillars]" and these resolutions, henceforward, guide the arts (298c3, 298d5–e3). Underlying popular rule is suspicion of the motives of those who possess some art or knowledge that most do not. Accordingly, the multitude, with a view to self-protection, establish rulers, selected by lot rather than merit, to supervise those who posses art (298e5–7). Moreover, each year these rulers must be scrutinized, made to respond to any charges brought by any citizen that they did not supervise the arts in accordance with those writings which, as fundamental law, must take precedence over any of their individual decisions (298e11–299a6).

In setting these limits, however, the community pays a price. The references to *kurbeis*, on which Solon's laws were originally written, and to naval battles with "long ships," old-fashioned warships, suggest the result of the limitations put on knowledge in this community (298d4).[43] The reliance on the old, the trusted, and the ancestral requires the community to look askance at innovation of any kind, political or technological. The Stranger brings the point home with startling clarity in his direct reference to the impending trial of Socrates. He states that anyone seeking knowledge of seamanship or medicine must be considered

> a kind of garrulous sophist and . . . on the grounds that he's corrupting different people younger than himself, and convincing them to engage in piloting and medicine not in conformity with the laws, but to rule with their own authority the ships and the sick, then anyone who wants can and is permitted to draw up an indictment and haul him before a—what do you call it?—court of justice. And if it's decided that he persuades either young or old contrary to the laws and what is written, to punish him with the extreme penalties, for he must in no way be wiser than the laws (299b6–c6).

Although the Stranger is unfamiliar with Athenian procedures, he can relate with precision the outcome of Socrates' trial. The man of theory grasps the trial as a necessity following from the nature of political life itself rather than as a historical event specific to Athens. The Stranger knows that the community must for the most part adhere to what is second best; it must limit the wisest or the best for the sake of life in common. But in this very portrayal of Socrates' situation, he suggests the limit of this claim. The community would have us believe that the only political alternatives are arbitrary rule by individuals or the absolute rule of law. This claim could hold true if the god rather than humans established the law. We reside, however, in the age of Zeus wherein all rule, including the rule of law, derives from humans. It cannot be the case, therefore, that no individual may be wiser than the laws (299c6).

As earlier the extreme claim for the rule of the wise without law brought forth young Socrates' objection, so too does this extreme claim for the rule of law. Young Socrates recognizes the community's need for the few outstanding in intellect, lamenting that limiting innovation would mean that "all the arts we have would completely perish" and life would prove to be "altogether unlivable" (299e5–9). He thus expresses that concern which gives intelligence a foothold in the community: Few wish to abandon the fruits of the intellect.[44] More generally, our very need for the arts implies that they cannot be limited in this way. In this Zeusian age we must have the latitude to respond to a harsh and changing nature. Despite their inclination to limit and if necessary destroy the few wise, the many are dependent on those few.[45]

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[42] While Miller (1980, 96) is right that the features of politics developed here refer to Athens, the Stranger also points to an enduring political possibility and so in the midst of this description the Stranger indicates his lack of familiarity with Athenian procedures (299c3).

[43] On this point, see Campbell (1988, 151n6) and Benardete (1984, III.154n39).

The Stranger responds to young Socrates' protest with a vigorous defense of law. But even in this defense he points to the need for the wise. For example, he leaves open the possibility that someone acting from motives other than "profit or private whim" who was "familiar" with the laws could alter them (300a4–6). Most important, he also makes clear that not just any writings are to be respected but rather those that "have been laid down on the basis of much trial and error, when certain advisors gave several pieces of advice in a neat and elegant way and persuaded the multitude to set them down" (300b2–4). There is a need at some point for the community to be informed by wisdom. Just as someone must have sought an art of piloting or medicine at some point, so it is with respect to the art of politics. Despite the desire and even the necessity of the political community to obscure its origins in human wisdom, the community still depends on the wisdom of "certain advisors" and it is possible, albeit dangerous, for other individuals to ask about the wisdom of the laws (300b2).

In the absence of divine providence we depend on the effectiveness of intelligence for our survival not to mention our flourishing. The possible range of outcomes for this endeavor is vast; if organized wrongly human life could be, as young Socrates says, "altogether unlivable" (299e8). Because of this potential for highly divergent outcomes, the difference in intelligence among humans becomes crucial. With respect to political life in particular, it matters who makes the law. The law and other institutions can preserve wisdom beyond the life of the wise individuals. These institutions, however, cannot dispense with such individuals. As the diversity and variability of the human things require that wisdom limit itself in the rule of law, so also do these qualities require that law should be informed by wisdom. In what follows the Stranger shows how wisdom informs law or, to use his term, what ought to guide our imitations.

This guidance emerges from the Stranger's comments on tyranny (301b10–c4). Tyranny is defined as imitation based on ignorance.[46] The tyrant's ignorance lies in his believing himself to be the true statesman who need not be fettered by laws, the ruler capable of ruling directly through wisdom alone. Ignorant of his ignorance, the tyrant neglects, whether unwillingly or out of a "certain kind of desire," that rule must usually be through law rather than direct, that imitation is necessary (300c2–3). But is imitation always necessary? Would it also be so for the true statesman? The Stranger is clear that all must "write up writings while they run after the traces of the truest regime" because "there is no king that comes to be in the cities . . . who's of the sort that naturally arises in hives—one who's right from the start exceptional in his body and his soul" (301e2–4, 301d8–e2).[47] The Stranger teaches about both human equality and inequality: that there are vast differences among humans but no human stands to another as a god, that these differences are not so great as to deny our shared humanity and the shared limitations intrinsic to it. This peculiar character of our diversity is the obstacle in the way of direct rule of the wise. Stated positively, it provides the rationale for the rule of law based on consent, that is, for political freedom.[48] In sum, it is the human *eidos*, which somehow comprises without reconciling these vast differences, that makes imitation necessary for every community.

In this light we can understand the question which the Stranger now addresses to young Socrates. He asks whether we should be "astonished as to how a city is something strong by nature" even though, many cities "do sink below like ships and perish and have perished and will go on perishing on account of the sorry state of their captains and crews" (302a5–8). The city is "strong by nature" in that it reflects and responds to human nature in its diversity. But precisely insofar as it does this its wholeness is in question. As with individual humans, the city's wholeness is not spontaneous; its order is a product of human intelligence. What keeps the city from perishing, from sinking into disorder—into "the sea, which is limitless, of dissimilarity"—is the wisdom of its captains and crews (273d6–e1). By nature we require political communities for our flourishing, but this very nature dictates that the political community is a human production. Most important, if such production is informed by wisdom, then it does not aim to approximate the one true regime which must be distinguished "from all the rest as a god from human beings" (303b4–5).[49] To do so is to emulate the defective rulers whose defect lies in their being ignorant of their ignorance (302a8–b3). Its production must rather be guided precisely by recognition of why imitation is necessary, why the community's wholeness is only approximate. This recognition, finally, differentiates the statesman from his "greatest imitators," "the sophists of sophists" (303c2–5). It is not that the statesman is true and genuine while the sophist is imitative, but rather that the statesman is guided by awareness of why imitation is unavoidable, why the perfectly harmonious community must be unavailable.

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[44] On this point, see Rosen (1995, 174). I am much less convinced than is Miller (1980, 99–100) that young Socrates' remark is an important sign of his philosophic progress.

[45] There are several indications in this context of such dependence, for example, the Stranger quoted a phrase from the *Iliad* which refers to the superior value of the physician's wisdom in the uncertainty of war (XI.514). Also, he lists seven pairs of arts with the first of each pair being a very narrow art while the second is a very expansive art. The list implies that the resolution of issues raised by the narrow art points to the more comprehensive inquiry, as the propriety of any particular law may lead to inquiries concerning justice in general. On this point, see Skemp (1987, 207–8n1).

[46] Although, he refers to the tyrant's desire and ignorance, the discussion bears much more on the latter than the former. The entire discussion of statesmanship reflects a diminished importance of moral virtue as one of its components.

[47] For Rosen (1995, 177–8) this passage substantiates his claim that political life, or rather "healthy" political life, is not regarded as natural in the *Statesman*. My view is that political life is natural, even or especially given its problematic character, because this character reflects human nature.

[48] On this point, see Griswold (1989, 161–2) and Crosson (1963, 32–6).

The Stranger's last typology reflects this understanding of the way in which wisdom should inform law.[50] The criterion of this typology is not the best regime but rather which of several regimes is least difficult to live with—freedom from oppression is an important consideration (302b5–6). In a departure from his initial typology, the Stranger places importance on the distinction of democracy into lawful and lawless, and he praises democracy as a whole as the best of the "intemperate" when it is without law and as the worst of the best when it is with law. With the focus on what is achievable in our age, the desirability of democracy, lawful and lawless, increases in comparison with both monarchy and aristocracy (302c1–2). There is a greater appreciation here of moderate achievements in politics, even of the mean in the sense of the mediocre; aiming for the "one right regime," even or especially when animated by a desire for justice, courts disaster.[51] The good of democracy depends as much on its institutions, which achieve moderation through the dispersal of power, as on the wisdom of its rulers (303a4–7).

This is not to say that the community can dispense with the guidance of active intelligence. To expect institutions alone to solve the political problem is to forget that the stability provided by the rule of law is always in need of adjustment, the harmony of the community is always approximate. But, likewise, when we look to statesmanship alone for our political salvation, we neglect the degree of diversity among humans and overestimate the sway of human understanding.[52] Underlying the Stranger's revised typology, his rehabilitation of the nonepistemic criteria of politics is, in fact, the recognition that we require two intrinsically different and irreducible types of rule—rule by wisdom and rule by law based on consent. And the Stranger's dialectical examination of these types of rule has allowed us to see that this requirement exists because politics, as a reflection of the irreducibly complex human *eidos*, must also involve *both* those qualities and concerns which all share, as well as those found in only a few individuals.

The irreducibility of these qualities and concerns dictates that political science must involve both discovery and production or, in other words, that there can be no *direct* application of our discovery of the nature of the human things to the plane of practice. It is this rift between general understanding and particular circumstances that makes *phronesis* necessary. The purpose of young Socrates' political education is, finally, to present to him the irreducibly complex object that underlies this rift, this limit on our knowledge. An appreciation of such an object could pierce that certitude of young Socrates which is one of the greatest obstacles to philosophy. Through the reflection on politics provided by the Eleatic Stranger, young Socrates could grasp the insight of his older namesake: the limitations on human understanding that necessitate a "second sailing," an alternative way of striving for the good that is available to us in full awareness of our distance from the best (300c2, see *Phaedo* 99c9–d1; *Philebus* 19c2–3).[53]

CONCLUSION

The successful rehabilitation of *phronesis* requires that we be able to distinguish its openness from arbitrariness. The conversation between the Eleatic Stranger and young Socrates shows how it may be possible both to preserve the latitude inherent in *phronesis* and to determine a nonarbitrary guide for its application. This possibility depends upon the discovery of the cause of this latitude, a cause which the Stranger locates in the human *eidos* itself.

Young Socrates' longest speech makes clear why he is the appropriate interlocutor for this conversation. In it we see the discrepancy between his preferred mode of understanding, on the one hand, and human diversity on the other. Reflection on this discrepancy could enable young Socrates to see that there are objects which elude abstract analysis, that neither his mathematical training nor the diaeretic method to which he is attracted accounts adequately for an *eidos* as complex as the human one. Such reflection could lead him to see the conditions that call for *phronesis* because it is precisely the tensions comprised somehow by the human *eidos*, tensions amplified by the disorderedness of our world, which substantiate the ineradicable need for *phronesis*. And it is precisely in these conditions of *phronesis* that the Stranger finds the possibility of a guide that is both nonconventional and nonreductive, consisting as it does in the nature of the human things understood as uniquely diverse and variable. In wisdom's self-limitation, in his praise of democracy, and, most generally, in showing the permanent need for wise judgment, the Stranger has shown the work of this guide as it informs *phronesis* without supplanting it.

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[49] I agree with Gill that the "error" which the Stranger corrects here is that communities come to believe their own laws are simply the best. But I disagree that this error is corrected by the ruler possessing "objective, non-relativized, ethico-political knowledge, which is also effective in practical action" (1995, 293). Gill recognizes that the Stranger states that the king will use laws and that human life without regimes characterizes the age of Cronus, but he attributes little importance to these considerations (p. 299n38). Likewise, Lane recognizes the importance of the rule of law but maintains finally that the "ultimate source of political authority would be the exercise of knowledge by the true statesman, should he ever appear" and "it is the possibility of his advent which must be prepared" (1995, 289, 291). These views take as a model the rule appropriate to the age of Cronus rather than to our age. This is the error the Stranger aims to correct. On this point, see Griswold (1989, 155–60).

[50] For Gill this typology is "irrelevant" because he gives little attention to the manner in which the Stranger corrects the error made by political communities (1995, 301). The Stranger refers to it as *parergon* (by-product) but this is because his ultimate aim is to educate young Socrates with respect to knowledge rather than politics.

[51] Miller (1980, 99–101) and Griswold (1989, 159–60) both recognize the value of the mean in this sense, although Miller still holds out the possibility of an epistemic statesmanship beyond this. Skemp writes that the Stranger's praise of democracy presents the "greatest practical acknowledgement of the merits of democracy to be found in Plato" (1987, 65). See also Annas (1995, xviii–xix) but cf. Mara (1981, 376–7). The praise of democracy does not follow only from its compatibility with philosophy, as is said about the treatment of democracy in the *Republic* (Bloom, 1991, 421, and Kraut, 1984, 227).

[52] *The* distinguishing mark of the true statesman is his awareness of this limit on his knowledge. Griswold (1989, 161–2) and Annas (1995, xx) show how this awareness points to the requirement of consent.

[53] Thus while I agree with Weiss that the statesman is an *epistemon* (a knowledgeable one), I disagree concerning the crucial content of the statesman's knowledge. In this respect Weiss's view is much akin to the views of Gill and Lane (1995, 221). Also, where Cropsey wants to emphasize the great difference between rule by the one with wisdom and the second-best, the rule of law, I want to emphasize the importance of recognizing this difference (1995, 129–32). On this point, see Zuckert (1996, 481).

The possibility of such a guide, articulated in the dialectical examination of the rule of wisdom and the rule of law, merits further study by political theorists engaged in the rehabilitation of *phronesis*. It is important, however, that such studies heed the importance of the Stranger's theoretical reflections. For while he does lead the young mathematician to engage in political reflections, these reflections serve the guiding purpose of the trilogy: to understand the character of human knowledge (285d4–6). Specifically, the Stranger conveys the complexity of the human *eidos* through reflections on politics, turning to politics as a window into the complex character of humanity and the world we inhabit. He takes young Socrates and himself down the Socratic path to the human things but, like Socrates, does not thereby circumscribe his view to the realm of practice.[54] His reflections show that to understand *phronesis* in the light of human nature and nature as a whole need not culminate in a distortion of the political sphere; far from denying the essential characteristics of *phronesis*, these reflections substantiate our need for it. And it is of particular importance for the contemporary attempt to rehabilitate *phronesis* to see that without such reflections the contemporary critique of the application of scientific reason to politics remains open to the charge that it rests only on a sentiment. But, as I hope to have shown, the need for *phronesis*, and its proper guide, may rest on firmer ground. In finding scientific reason not wholly adequate to the understanding of politics, we can appeal beyond sentiment if we see *phronesis* as inseparable from an understanding of humanity and its place in the whole. Only then can we explain the cause of this inadequacy and only then might we find the proper guide for the use of wisdom.

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[54] See note 6 above. Several commentators have recognized the ever-increasing Socratism of the *Statesman*. See Campbell (1988, xvi–xviii); Skemp (1987, 23); Scodel (1987, 151) and Weiss (1995, 214–50). None of these understands this progress in the way I have presented it. Perhaps Socrates' silence connotes his approval of a discussion which can be said to result in his philosophic vindication. (But cf. Cropsey 1995, 139–40.)

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Private Property versus Markets: Democratic and Communitarian Critiques of Capitalism

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This essay assesses communitarian and democratic critiques of capitalist economies. Distinguishing them are sharply contrasting evaluations of markets and private property. Communitarian critics of capitalism trace its moral failure to the marketplace. Drawing on Aristotle's normative economics, this school maintains that production for gain corrodes society's moral fabric. I defend the democratic approach. Democratic critics accept the modern claim that markets are both efficient and liberating. Capitalist ownership relations are another matter, indicted because they constitute a form of private power over people's lives. I reconstruct the ethical core of the democratic school and contend that it offers a better understanding of the most objectionable aspects of capitalist economies while avoiding the dangers inherent in the communitarian approach.

The modern separation of economic from moral concerns has never been secure. Normative approaches have contested the reduction of economics to ethically neutral questions about the efficient allocation of scarce goods with diverse uses. These theories repay closer analysis. First, they offer the conceptual resources to sustain economic criticism at a time when capitalism's triumph threatens to foreclose questions about alternative ways of organizing our interchange with nature. Second, and just as important, these theories address debates central to our discipline, informing our understanding of traditional and capitalist economies, the distinction between public and private, and the moral character of the market and private property.

There are, however, fundamentally incompatible normative approaches to economic life. One of my aims is to challenge the prevailing theory of the moral economy, which is communitarian in orientation; the object of its criticism is the modern marketplace. I will defend an alternative approach, the democratic critique, which differentiates markets from capitalist ownership, inculpating only the latter. Although prevalent in the writings of a number of social theorists, its organizing principles have not been explicitly formulated. In this essay, I reconstruct its ethical core and demonstrate that it offers a better conception of the problematic standing of capitalist economies while avoiding the dangers inherent in the competing school.

Communitarian critiques of capitalism appeal to an economy organized to sustain a substantive conception of the good life. Their theoretical nucleus is the contrast between premodern and modern economies, distinguished in terms of the formation of markets. Aristotelian in provenance, its conceptual dichotomies underscore the radical discontinuities attending the rise of market society. The disjuncture between the two worlds on either side of the "great transformation" has been variously represented: status and contract (Maine 1963); *Gemeinschaft* and *Gesellschaft* (Tönnies 1955); mechanical and organic solidarity (Durkheim 1964); embedded and disembedded economies (Polanyi 1968); production for use and production for exchange (Sahlins 1972). Beyond providing an understanding of the gulf separating traditional from modern societies, these contrasts convey a sense of the profound moral loss accompanying the transformation (Evans-Pritchard 1967, ix; Tönnies 1955, 37–9).

The critical intention of this school develops along explanatory and normative dimensions (see Booth 1994). First, it challenges the heuristic validity of modern economic science, limiting its explanatory purchase to the exceptional formation of a self-regulating economy. Only modern societies can be meaningfully understood in the vocabulary of the marketplace. To represent premodern economies in economic terms, to generalize a distinctly modern propensity to truck, barter, and exchange, is not merely anachronistic; it radically distorts the nature of premodern economies, whose institutional nucleus is the household. In orienting us to its world, the communitarian approach makes use of Aristotle's (1984, 1256a–1259a) economic analysis. Aristotle does not think, as we do, of the economy as an independent, specialized sphere governed by its own laws (the price mechanism), patterns of behavior (exchange), and motives (gain). *Oikonomia*, or household management, is oriented to and governed by noneconomic purposes. Like education, drama, and warfare, provisioning is an aspect of the community's life and so is subject to its normative concerns and political control.

Second, the historical perspective of premarket economies supplies the angle of vision for an incisive moral evaluation of market society. Aristotle's distinction between production for use and production for exchange serves to focus this critique. Although concerned with the acquisition of wealth, the household is primarily responsible for its proper use. Acquisition for the sake of gain confuses the necessary means of life with life's higher purpose. The marketplace epitomizes a prodigious human attempt to master economic necessity, which, much like the fate of King Midas, perversely subordinates human beings all the more to

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it. Its requirements corrode the society's moral fabric—community dissolves into the cash nexus, need is subordinated to profit, culture gives way to acquisition, leisure is sacrificed to work.

Democratic approaches represent a different way of thinking about our relationship with nature. Their hallmark is a concern with the moment of ruling in the process of material provisioning. That which distinguishes them from communitarian accounts is the shift in analytical focus from the marketplace to the property regime. Three claims characterize democratic critiques of the economy. First, property is essentially a form of rule, endowing owners with rights to command the services of others. Second, a fateful continuity underlies the transition from traditional to capitalist economies. Modern property fails to effect a fundamental break with traditional ownership relations. Capitalism is indicted because it preserves the defining feature of premodern property, generating political power to determine the risks and destinations of other people (Walzer 1983, 291). Third, this school disassociates markets from property: Capitalist ownership, not the market, constitutes the morally compromised aspect of the modern economy.[1]

This essay is divided into two main parts. The first explores the organization of the premodern economy. Democratic and communitarian perspectives draw significant normative conclusions from analyses of traditional material life, but they appeal to very different aspects of it. The second section examines the transition from premodern to capitalist societies. In the democratic view, the changing nexus of public and private attending the formation of the modern economy reflects not so much the creation of an autonomous market as the privatization of economic power. In addressing the normative implications of this process, I argue that we must rethink not only the scope of society's governance over the economy but also the distribution of power within the workplace.

THE PREMODERN ECONOMY

Premodern Property Relations

The unifying theme of communitarian studies, drawn from Polanyi's theory of the embedded economy, is the claim that premodern material life was submerged in various kinds of noneconomic relations. Motives other than profit, such as prestige or communal solidarity, guided economic activity, and practices other than exchange, in particular reciprocity and redistribution, structured the economy (Polanyi 1968, 3–19, 139–57). To underscore the contrast between traditional community and modern market, however, Polanyi (1968, 16) must

> deliberately disregard . . . the vital distinction between homogeneous and stratified societies, i.e., societies which are on the whole socially unified, and such as are split into rulers and ruled. Though the relative status of slaves and masters may be worlds apart from that of the free and equal members of some hunting tribes, . . . the organization of the economic system may still be based on the same principles.

Omitted from the analysis, then, is the economy's political aspect, the terms on which economic relations are experienced and contested by rulers and ruled. Communitarians, notes Booth (1994, 661), appeal to the premodern world

> as a mirror in which we may more readily see the flaws of our own, and in the course of doing that, they abstract from its particular binding cement, from its hierarchy and domination. . . . Yet embeddedness is hardly intelligible apart from that context, and once it is extracted from its specificity it loses much of its conceptual substance, becoming an ill-defined, if seductive, perception of a 'thick' community.

Needless to say, Polanyi is fully aware of the dimensions of power in premodern and modern economies. The problem is that the process of ruling escapes the conceptual dichotomies that organize his work.[2]

To expose the economy's political face, democratic approaches recast Polanyi's theory of the embedded economy. Noneconomic relationships governed the material life of premodern societies. Embeddedness, however, refers primarily to the property regime constituting the relations of dominance and subjection. Premodern economies were distinguished by a characteristic form of ownership (Brenner 1986, 27–32).[3] Its basic units were the individual households of lords and peasants. The peasant holding entailed direct access to the household's means of provisioning; domestic relations comprised its relations of production (Wolf 1966, 13). The aim of the household was self-sufficiency, to satisfy its customary needs from its own resources, reproduced by the family's labor force. Households were not, however, autarkic. "Nucleated" villages, clusters of households surrounded by agricultural land worked by villagers, formed the dominant pattern of settlement. The household's property rights were neither absolute nor exclusive. Ownership encompassed a wide variety of coincidental use rights, shaped by custom, that regulated overlapping claims to the same resources. Even the landlord's estate was normally farmed in scattered plots by peasant labor and so was entangled in the customary practices and rhythms of the village.

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[1] Seminal works in this school include Braverman 1974; Brenner 1986, 1987; Hilton 1973, 1975; Lindblom 1977; Macpherson 1973; Nove 1991; Thompson 1968, 1991; Walzer 1983; and Wood 1995. The distinction between markets and property highlights the contrast between communitarian and democratic criticism. I should add that most, but not all, of the theorists explored here make this distinction explicit; and most, but not all, agree that property, and not markets, is morally culpable.

[2] See also Sahlins (1972) and Scott (1976). Although both adopt Polanyi's theoretical framework to explain the distinction between premodern and modern economies, their actual analyses give a prominent place to political factors, especially economic power. The thesis that property relations rather than markets provide a surer conceptual lens to capture the dimensions of power in premodern economies and in the transition to capitalism is the centerpiece of Brenner's (1986, 1987) work.

[3] Although they share some features with economies of hunters and gatherers, the economies I discuss are largely confined to those of settled peasant agriculturists. The reason for this focus is that primitive communities, in contrast to communities of peasant cultivators, lack a developed form of economic rule (Sahlins 1972, 130–48, 204–10; Wolf 1966, 3–4).

Although villages were virtually self-sufficient, they were not autonomous. Peasant holdings were normally attached to an overlord's household, establishing the nexus between ruler and subjects. Because peasant households possessed primary access to their own productive resources, rulers secured control over their domestic economy by political means, instituting direct, enforceable claims to their services. The subordinate condition of premodern producers assumed various forms of juridical dependence or political subjection—slavery, debt bondage, serfdom, corvée labor, and so on. Rent, tax, and tribute comprised the main forms of appropriation. The specific way in which an economic surplus was levied from the subject producers—for example, by means of a centralized palace bureaucracy or the institution of lordship—distinguishes different types of premodern economic formations (Anderson 1974, 401–9).

The process of ruling was one aspect of the relationship between lords and producers; equally important was its private character (Wood 1995, 31–4). A central feature of Western history is the progressive differentiation between public and private spheres of activity. Already manifest in Homeric Greece, this development can only partially be interpreted as a growing division between politics and economics. What took place was the separation of private from public domains of rule (Finley 1981, 213–32; Wood 1988, 85–9, 90–2). Although the state is a sovereign political community, noted Aristotle, the lord's household is an individual sphere of jurisdiction. The state is by no means entitled to supplant the authority of husbands, fathers, or masters over their proper subjects (Aristotle 1984, 1252a7–9). The landlord's estate was vested with certain political entailments, including control over dependent producers. Duby (1968, 220–1) provides an account of the political economy of the lord's household in this description of the early medieval *familia*:

> The most highly prized of a lord's possessions . . . was to be found only in the 'family.' The complex social group known as the 'family' included first of all the domestics. It contained also peasants set up in their own homes; some, but not all, of the lord's tenants; freeholders who had become clients; and occasionally tenants of other lords. . . . He considered them 'his men.' What then did he expect from them? Above all he expected them to work. The eleventh- and twelfth-century 'family' constituted before all else a reservoir of manpower into which the master could dip freely for help in the cultivation of his demesne. The first duty of a dependent consisted in 'service,' by which was meant that he was bound to fulfill all his lord's orders. To make the most of the labor force which was entirely at his disposal the lord kept it as far as possible under his own roof.

The overlord's estate formed the terrain on which rulers and ruled disputed the terms of their relationship. Premodern economies were indeed characterized by production for use. The balance of political power, however, conditioned the size and distribution of surpluses produced by the peasant economy. Peasant households faced the dominant classes as an organized village. Although not a community of equals, poor smallholders and wealthier peasants were still part of the same social group, responsible for administering their common affairs. The continuous coordination of tasks formed the practical basis of collective action. "Over time and over space," writes Thompson (1991, 107), "the users of commons . . . developed a rich variety of institutions and community sanctions which . . . effected restraints and stints upon use." Village self-rule was an integral aspect of peasant life:

> In the setting of the village community or peasant commune, the peasant reaches a level of nearly total social self-sufficiency. The appropriation and division of land, marriage, social and religious needs are generally taken care of at the village level. A common interest in commune rights as well as in providing for productive activity requiring the participation of more than one family generates co-operation, generally coupled with some type of grassroots democracy. . . . All this makes the word *mir* (meaning 'the world' or 'peace') used by the Russian peasants to refer to their village commune, a significant description of its function (Shanin 1971, 244).

The Moral Economy of Work

In changing the analytical focus of the embedded economy to its political plane, democratic approaches reveal a different normative vision of material life. One striking blind spot in the communitarian critique is its failure to attend to what Booth (1994, 660) calls the "domination moment" in the premodern economy. This assessment, however, is incomplete. For among the most enduring cultural achievements of premodern smallholders is a conception of emancipation, the idea of freedom as masterlessness, born out of their struggles against the rule of landlords and the state. The primary intent of democratic studies is to give voice to the traditions of the village. Its normative purchase lies in the recovery of the villagers' customary consciousness of work.

In contrast to the ruling classes, whose norms stressed their exemption from economic activities, peasants were primarily engaged in organizing production. Accordingly, their resistance implicated the very practices that constituted the economy. Smallholders expected recognition of their independent worth, an understanding that they were not merely instruments at the lords' disposal, akin to domesticated animals or tools. "This feeling," writes Hilton (1973, 40), "lay behind the objections to the acquittal of rent obligations in the form of labor services on the lord's demesne, though other factors, such as the equation of forced labor under the coercion of the lord's bailiff with serfdom or slavery, were involved as well."[4] Reflecting a deeply rooted aspiration to be attached to no household but their own, peasant struggles called into question the division between ruling and working. While their lords equated economic freedom with leisure, peasants equated free status with independence, drawing a sharp distinction between working for their own sake and for that of their lords (Hilton 1973, 38–75; Wood 1995, 187–92). Their aim was to protect a sphere of free activity within what their rulers considered a realm of necessity.[5]

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[4] This norm is nearly universal among subject classes. Within the harsh constraints of American slavery, observes Genovese (1976, 140), slaves nonetheless "drew their own lines, asserted rights, and preserved their self-respect." Perhaps the moral resistance of slaves explains Aristotle's (1962, 1161b5–10) reticence to defend his own

Peasant life observed its own economic routines (Sahlins 1972, 14–32, 51–69; Thomas 1964, 50–4; Thompson 1991, 355–61, 370–82). In comparison to modern wage laborers, premodern producers enjoyed more leisure and a more leisurely way of working. The available work force was normally underemployed. Production was oriented to the requirements of the task, and work ceased when livelihood was secured. The means of production were frequently homespun; households wielded their own tools, and knowledge of the required skills was widely available. Schedules reflected religious and secular traditions and practices, with no clear demarcation between economic and noneconomic activities. Work was unintensive and irregular—seasonal, short, flexible, and subject to all manner of interruption. This is not to say that productive activities were disrupted by extraneous cultural demands. Work had its own internal constraint, self-sufficiency; beyond it, time was available for other purposes.

Juxtaposing household and market, communitarian studies also invoke a moral world in which work serves human ends rather than economic imperatives. Without an analysis of the political dimensions of material life, however, the communitarian account of traditional work patterns remains deficient. These patterns cannot be explained by distinguishing production for use from production for profit. The absence of subjection, not the absence of markets, determined the ability of smallholders to control their own work and lives. Sahlins's (1972, 5–6, 27–9, 34–9) analysis of the contrast between paleolithic and neolithic economies provides an instructive illustration of the material consequences of rule. Hunters and gatherers, he argues, formed the original leisured societies; the transition to settled agriculture sharply increased the burdens of work. The principal reason lay in the imposition of the necessity to work for others. Primitive producers were exempt from the tribute, taxes, and rents normally levied on settled cultivators to underwrite the living standards of ruling classes (Sahlins 1972, 131–2, 147–8; Wolf 1966, 2–4, 9–10). To varying degrees in different times and places, peasant villages were compelled to sustain superstructures of aristocracy, state, church, and towns. They were able to preserve their customary modes of work and leisure only to the extent that they succeeded in resisting or redressing the encroachments of rulers.[6]

Property relations explain the nature of premodern economic development (Brenner 1986, 1987; Sahlins 1972, 130–48). The organization of political power determined the possibilities for growth. By compelling cultivators to produce beyond their customary requirements, especially by employing the family's idle labor force, rent obligations forced peasant households to improve the productive capacity of the village. But only up to a point, for economic growth depended upon the ways in which rulers used the surpluses they extracted. Funds returned to the village in the form of productive investments, such as for land reclamation or irrigation, underwrote economic development. Premodern ruling classes, however, employed the lion's share of their income to preserve and improve the means of extracting additional income, to reproduce the political apparatus protecting their privileged position. This array of outlays, mainly for war and largesse, generally depleted the material resources of the village. Beyond the barriers set by the productive potential of the household, landlord exactions eventually crossed the limits of economic safety, precipitating the characteristic crises of premodern economies—repeated harvest shortfalls followed by demographic collapse.

The Moral Economy of Subsistence

Various redistributive practices protected households against the dangers of economic failure. Their governing principle was the reciprocal exchange of gifts (Mauss 1967). Occupying a middle ground between altruism and self-interest, presentations of goods and services engendered obligations to return gifts of comparable value at another date. As assistance given and assistance repaid, they provided a form of social insurance against hardship (Scott 1976, 167–76). Among unequals, debts took the form of patron-client relations: The subject's obligation to serve was requited by the ruler's duty to provide timely support. At a minimum, peasants expected lords not to threaten their subsistence fund. At a maximum, they expected material contributions in securing their common welfare. Among equals, gifts took the form of village mutuality. Obligations were balanced, but only over the long run: Although one household immediately benefited from a transaction, at another time it was others' turn to benefit by chance shortages. The willingness of villagers to cooperate was rooted in their shared predica-

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equation of slaves with instruments, recommending instead that master-slave relationships be based on friendship rather than sheer coercion.

[5] Aristotle provided the most influential exposition equating work with enslavement by necessity. Communitarian and democratic approaches yield contrasting evaluations of Aristotle's normative economics. Whereas the former appeals to his critique of markets (Polanyi 1968, 95–115), the latter focuses on his contempt for labor and those who work (Wood 1988, 126–37; see also Murphy 1993, 87–112). Contemporary scholarship has disputed the view that ancient smallholders defined the freedom to which they aspired in Aristotelian terms (see Wood 1988, 137–45). Indeed, it is more plausible to suggest that pride in one's economic independence was prevalent in a society in which peasants and artisans were citizens.

[6] Among the most successful were ancient Athenian smallholders. Reversing the commonplace notion that slavery supported the political activities of a leisured citizenry, Wood (1988, 109) argues that peasants and artisans increased the time available for politics by their ability to restrict the time traditionally spent working for their aristocratic compatriots.

ment. Normally on the margins of subsistence, successful households presented goods and services to their more unfortunate brethren, in the expectation of receiving appropriate succor when needed (Scott 1976, 5; Wolf 1966, 80).

Village conceptions of minimum subsistence rights are the centerpiece of communitarian studies of the premodern economy (Scott 1976, 176–9). As part of their critical apparatus against the marketplace, communitarian approaches stress the moral solidarity of the village, the predominance of production for a livelihood over production for gain. Democratic analyses amend this account: Establishing a boundary between communal and private provisioning is an essential political function binding all communities, traditional and modern (Moore 1978, 3–48; Walzer 1983, 64–94). Grounded in the elemental fact that humans must cooperate to survive, societies are charged with the responsibility of securing their common conditions of life. The obligations forged by gifts create and regulate a distinctive domain of security and welfare. Material hardship defines its scope: Members must not be allowed to fall below a minimum. The moral requirement to do one's share encompasses necessities as well as the funds needed to fulfill social roles, but its imperatives vary directly with the urgency of needs. Reciprocal gifts involve different sorts of goods and services, but obligations are particularly pressing for the means of livelihood. Private provisioning, sale for profit, is less culpable, the less goods are necessary for subsistence (Sahlins 1972, 210–9).

The redistributive functions of premodern villages, then, do not distinguish premodern from modern economies. More accurately, they constitute historically specific definitions of communal provisioning (Moore 1978, 22).[7] Drawing the boundaries of security and welfare is in the first place a political task. The terms of association determine what is to be communally provided; beyond that lies the purview of individual provisioning. "Village egalitarianism," Scott (1976, 40) observes, "is conservative not radical; it claims that all should have a place, a living, not that all should be equal." What constitutes a living is continually renegotiated among the villagers. The conflict between what households owe themselves and what they owe the community is commonplace (Sahlins 1972, 124–30). Accordingly, maintaining the domain of security and welfare requires political coercion. Gifts are not entirely voluntary; they must be enforced (Scott 1976, 40–4). Providers of essential goods and services—millers and bakers, for example—must be constrained to serve the community, to work for fair allowance and not profit (Thompson 1991, 194).

Communal provisioning is necessarily redistributive. Social insurance benefits the most vulnerable, and its principles define the moral economy of the poor:

> Here ... is a more precise account of the social contract: it is an agreement to redistribute the resources of the members in accordance with some shared understanding of their needs, subject to ongoing political determination in detail. The contract is a moral bond. It connects the strong and the weak, the lucky and the unlucky, the rich and the poor, creating a union that transcends all differences of interest.... Arguments about communal provision are, at the deepest level, interpretations of that union (Walzer 1983, 82–3).

Finally, establishing and patrolling the domain of security and welfare was predicated on the political power of the village and, indeed, of the poor within the village. Only successful resistance against their rulers secured sufficient resources to safeguard the villagers' conceptions of customary usage. One common manifestation of subsistence rights was the doctrine of just price, adjusting the cost of staples, especially bread, to the purchasing power of the poor. Popular protests aimed at "setting the price" were in fact actions by the poor to enforce their understanding of welfare and administer its distributive norms. They policed the boundary between public and private provisioning: Having forced providers to sell at the just price, the crowd often returned the proceeds to the rightful owners (Thompson 1991, 229–33).

The Normative Sources of Peasant Rebellion

An implicit contract governed the relationship between classes. Their pact was not, of course, formally written in constitutions. Rather, "what takes place ... is a continual probing on the part of rulers and subjects to find out what they can get away with, to test and discover the limits of obedience and disobedience" (Moore 1978, 18). Lords and peasants worked out their economic relations in terms of conflicting definitions of usage rights, supported by appeals to tradition and sanctioned by manifest force. "Custom," writes Thompson (1991, 110), "may ... be seen as a place of class conflict, at the interface between agrarian practice and political power." The reciprocity between rulers and subjects was hierarchical; its working principle was paternalism. Lords were expected to safeguard the welfare of their subjects; subjects, in turn, consented to transfer goods and services to their lords. On the one side, the privileges of rank were matched by the obligations of nobility; on the other, the deference of subjects was matched by customary expectations of support.

Paternalism had a strong ideological accent. Underlying it was the studied exercise of power. Landlord generosity was always something less than liberal; reciprocal relations between lords and peasants were sharply asymmetrical. Nevertheless, for both rulers and subjects, the paternalist ideal provided a performance standard that stabilized premodern political power, converting it into legitimate authority (Scott 1976, 181). "Unequal as were the terms of power in this conflict, yet power [had to] submit to some constraints, not only because custom had juridical endorsement and could itself be a 'property', but also because power might bring itself into danger if abuse of customary rights outraged the populace" (Thompson 1991, 110–1). Rule was vindicated by its manifest contribution to the common welfare, especially because the state was simultaneously the lord's household, where power was exclusively held and privately exercised (Scott 1976, 184–5). The characteristic form of exploitation was rule which served the lord's own ends at the expense of the common good. Dissolving the reciprocal claims to deference, it justified resistance.

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[7] Rawls's "difference principle," redistributing the effects of brute luck on economic well-being, is a modern representation of the traditional gift relationship (see 1971, 72–5).

Peasant defiance has been characterized as "backward looking," essentially rebellions of consumers in defense of paternalism (Scott 1976, 11, 189). What this means is that peasants' ideas of legitimacy were drawn from their rulers' norms of the virtues and sins of the various orders of society. Rulers were blamed for their failure to discharge the duties that tradition and mutual obligation demanded of them. Writing about medieval peasant movements, Hilton (1973, 118) notes that rebellions were typically precipitated by members of the ruling class whose behavior "altered the customary relationships or disappointed normal expectations, to the detriment of the peasant class as a whole, rich and poor." Peasants perceived alterations in the social fabric from a conservative standpoint: "They cannot accept the abandonment of traditional roles by any one of the orders of the society—whose basic structure they do not . . . challenge" (p. 119). Addressed to deviant individuals or groups, their moral criticism upheld the order. The chief aims of peasant rebels were normally confined to removing offenders from power. And their main interest was to protect their roles as "consumers rather than producers. Except where communal land had been appropriated by local notables, the demand for the redistribution of land itself was strikingly absent" (Scott 1976, 11).

Embedded in this respect for custom, however, was a repudiation of class rule. As Scott (1976, 182) points out, "to say that there are norms in . . . power relationships is not to deny that . . . they may be exploitative. . . . There is . . . no contradiction between the insistence on those remaining rights that exist within a constraining context and the recognition that the context is itself unfair." Once the ordinary producers made protection of their customary practices the touchstone of political legitimacy, failure by the lords to honor their obligations justified any remedy, however radical. Peasant uprisings made this potential manifest: Resistance against particular lords was transformed into a rebellion against lordship; the defense of paternalism became a critique of subordination, a rejection of homage or service of any kind (Hilton 1973, 130–4, 220–32; Scott 1976, 191–2).[8] This egalitarian outlook, renunciating all reciprocal duties between rulers and subjects, is succinctly conveyed by a commonplace adage among peasant rebels in medieval Europe: "Whan Adam dalf and Eve span, wo was thanne a gentilman" (Hilton 1973, 211–2, 222).

Nor was the peasants' normative standpoint merely that of petty consumers. Although peasants rarely sought to redistribute land, they struggled, with some success, to redistribute political power, to wrest from their overlords jurisdictional authority to make binding decisions regarding their persons and possessions. The struggles of smallholders to secure the autonomy of the village reflected a fundamental interest in maintaining a viable independence. Radical peasant protests rejected the paternalist division between those who ruled and those who worked, giving rise to the ideal of the independent producer. Concluding his study of peasant movements in medieval Europe, Hilton (1973, 235) notes "that the concept of the freeman, owing no obligation, not even deference, to an overlord, is one of the most important if intangible legacies of medieval peasants to the modern world" (see also Marx 1990, 927).[9]

No peasant community ever succeeded in fully emancipating itself from service to lord and state. On the contrary, peasants were the primary casualties of modernization. If the advent of capitalism eliminated the peasantry as a class, however, its moral vocabulary survived to become an integral part of a powerful critique of capitalist rule (Scott 1976, 240; Thompson 1968, 834–915).

MARKET AND PROPERTY IN THE CAPITALIST ECONOMY

The contrast between embedded and disembedded economies is meant to conceptualize the uniqueness of capitalist society. The modern system of self-regulating markets did not merely reorganize premodern material life. Its imperatives assumed a privileged role in organizing all society. On the one hand, markets wrested from the community responsibility for human provisioning. Land and labor were transformed into commodities, compelling all to derive their livelihood from purchase and sale. On the other hand, market forces pressed outward, progressively subordinating noneconomic domains—the household, politics, culture—to their requirements. Permitted to pursue its own blind laws, the market system would have devastating consequences:

> To allow the market mechanism to be sole director of the fate of human beings and their natural environment . . . would result in the demolition of society. For the alleged commodity 'labor power' cannot be shoved about, used indiscriminately, or even left unused, without affecting also the human individual who happens to be the bearer of this peculiar commodity. . . . Robbed of the protective covering of cultural institutions, human beings would perish from

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[8] This is a common pattern of social criticism: Normative judgments that begin by holding rulers accountable to their own performance standards deepen by their own immanent logic to implicate those standards themselves (see Walzer 1994, 41–61).

[9] The notion of masterlessness is more generally grounded in the peasant experience (Wolf 1966, 106–9); Wood (1988, 126–37) traces it to the Greek *demos*. Patterson (1991) locates its genesis in the ancient experience of enslavement. But the emancipation of the Athenian smallholder preceded slavery, helping to reconcile the propertied classes to the new democratic regime by supplying them with an alternative source of dependent labor (Finley 1980, 67–92). The contrast between freedom and slavery emerged in stark relief once peasants ruled out all other forms of subjection, especially debt bondage.

> the effects of social exposure; they would die as the victims of acute social dislocation (Polanyi 1957, 73).

Booth (1994, 661) is right to be skeptical of this story. Just as the theory of the embedded economy overlooks the moment of domination in traditional societies, so does the account of the disembedded economy ignore the element of emancipation in modern economies. Markets embody a normative commitment to regard each person's interests equally, to protect a domain of self-determination wherein individuals are free to pursue their projects according to their own preferences.

Missing from Booth's assessment, however, is an account of capitalism's property regime. Liberal theory establishes a valid connection between markets and liberty. Capitalist ownership relations are another matter. In Lindblom's (1977, 46) words, "the traditional liberal argument is incomplete unless it defends private property as itself consistent with freedom.... It is simply blind to the implications for freedom of Proudhon's 'Property is theft!' as well as to implications of less extreme interpretations of how property is established and maintained." This is the distinguishing thesis of democratic critiques of the modern economy: Marketplace and private property, in the form of capitalist ownership of the workplace, constitute distinct economic domains, and only the latter is morally problematic.

To be sure, the normative barrier between capitalist property and markets cannot be sustained across the whole range of economic resources. In Polanyi's view, the moral failure of capitalist societies is grounded in the fact that markets usurp control over labor, land, and money. The constituent elements of material life—human beings and nature—and the universal medium of exchange cannot be counted as commodities. "The postulate that anything that is bought and sold must have been produced for sale is emphatically untrue in regard to them.... The commodity description of labor, land, and money is entirely fictitious" (Polanyi 1957, 72). Democratic evaluations of the marketplace, however, distinguish money markets from markets in goods and labor, including raw materials and machinery. Under capitalism, markets serve two distinct ends: They allocate existing resources, and they determine the course and rate of future development (Schweickart 1996, 71). The market in money, as a way of raising and dispensing investment funds, produces substantial concentrations of private economic power. Investors constitute a privileged class in capitalist societies. The need to maintain investors' confidence severely constrains the scope of democratic policymaking because the quality and pace of economic growth are crucially predicated on their decisions (Lindblom 1977, 170–88).[10] Accordingly, democratic alternatives to capitalism envision supplementing or replacing financial markets with public processes of generating and allocating funds for new investment.[11] In contrast, markets in goods and labor, bringing together firms, workers, and consumers, constitute essential prerequisites of personal liberty (Walzer 1983, 104–5, 116). Capitalism's moral failure is not rooted in the operation of these markets but in its characteristic property regime.

Defining Capitalism

Central to democratic approaches is the claim that profound continuities underlie the transition from premodern to modern forms of property.

> Private government survives in the post-feudal economy. Capitalist ownership still generates political power, if not in the market, where blocked exchanges set limits at least on the legitimate uses of property, then in the factory itself, where work seems to require a certain discipline. Who disciplines whom? It is a central feature of a capitalist economy that owners discipline non-owners (Walzer 1983, 294).

Modern property does not merely transform resources into fully marketable commodities. More important, ownership achieves an unprecedented control over production. The market relations regulating the economy outside the workplace do not penetrate within, where owners determine how work is planned, scheduled, and distributed. Having purchased labor services in the marketplace, owners or their managers become bosses, commanders of troops. They do not bargain over the amount or kind of work to be done; they order it.

Adam Smith's descriptions of exchange, on the one hand, and of the allocation of work in the pin manufactory, on the other, reveal the disjuncture between marketplace and workplace. Producers inside the plant are not free to "truck, barter, and exchange one thing for another," or to decide on the amount and type of work they will do, the way "the butcher, the brewer, or the baker" are free to take advantage of an opportune situation (Smith 1937, 13, 14). Private property places the factory not only beyond public purview but also beyond the market. Its workers are tightly organized and regimented by the owner, each confined to repeat "one simple operation,... making this operation the employment of his life" (Smith 1937, 7).

For democratic theorists, the hallmark of the transition to capitalism was the change in property relations (Brenner 1986, 1987; Macpherson 1973, 120–33). Premodern terms of access to productive resources were fundamentally redefined. Rights founded on ancient practice, protected by custom, gave way to prescriptive rights to things, protected by formal law. Shared claims to the same resources, limited to specific usages, were transformed into absolute, individual claims to exclude others from their use or benefit. Contemporaries summed up these changes under the notion of "enclosing," a process whereby the lords of manors or their farmers replaced the open-field system of agriculture with its intermingled strips and collective rules of cultivation, with cultivation in closed fields, at the discretion of the individual owner (Tawney 1912, 216–30).

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[10] The private power of investors is tied to their structural role in making funds available, independently of any role they may play in managing the enterprise. Accordingly, investors remain privileged even if they do not control the workplace. From a democratic standpoint, interest income, the return paid on safe loans of money, raises a host of normative concerns, since it is unrelated to any productive contribution, including risk and innovation. The financial reward accruing to inert owners invites comparison with feudal rents (see Galbraith 1971, 378).

[11] Public control over banking and finance raises extremely vexing questions: What role, if any, would remain for capital markets? What compromises might obtain between public authorities and private investors, between centralized and decentralized planning, and between government investment and investment financied from enterprise funds? For alternative suggestions, see Nove 1991, 220–1, 238–45; Schweickart 1996, 70–6, 131–6.

Locke's defense of private appropriation, his rejection of the claims of common right usage, plays a pivotal role in this account of the transition to capitalism (see Wood 1984). What Locke's text sanctions is the privatization of property, withdrawing access to resources—open fields, wastes, forests—from the jurisdiction of the village:

> The grass my horse has bit; the turfs my servant has cut; and the ore I have digged in any place, where I have right to them in common with others, become my property, without the assignation or consent of any body. The labor that was mine, removing them out of that common state they were in, hath fixed my property in them (Locke 1980, 28).

Only enclosure of common lands protected by formal title required consent; no agreement is otherwise required to enclose the commons, which may be individually appropriated "without the assignation or consent of any body." Locke denies that customary usage has any force against private appropriation, which he places beyond the province of communal authority. This includes appropriation of the free gifts of nature, resources not produced by labor, contravening what Bloch (1966, 182) considers one of the core principles of ancient practice: "the feeling that wild places and water, untouched by human hand, could not be appropriated by any individual."

Privatization of property was one aspect of the transition to capitalism. The other was its concentration in the hands of the wealthy. The increasingly absolute definition of property changed the use rights of both lords and peasants into "virtually unlimited and saleable rights to things" (Macpherson 1973, 128). But feudal tenures were not symmetrically abolished. The lords retained use and benefit of resources, while "at the same time, in the name of absolute individual property, the common and use rights of the 'lower orders' were eroded" (Thompson 1991, 161). The open-field system had been under pressure since the fifteenth century, as cottagers themselves engaged in small acts of enclosure in response to growing market opportunities. But "the effect of enclosure by the lord of a manor or large farmer was usually very different from that of enclosure by the customary tenants. The latter was a slow process of attrition, which went on quietly from one generation to another. . . . The former was frequently an invasion" (Tawney 1912, 151).

Agrarian rebellions are normally characterized as protests against the encroachment of the market. Smallholders were not, however, implacable foes of commercial principles (more on this in what follows). What they resisted was expropriation. They rejected Locke's (1980, 36–7) reading, in which the passage from the earliest ages of the world, when households had independent access to their means of production, to modern times, when improving landlords closed off the commons, is accomplished without loss of status. Juridically free, they were dispossesed. The loss of access to forests, wasteland, and common pasturage undermined the bedrock of their economic independence. The Levellers, many of whom were petty producers themselves, represented democratic currents in the seventeenth century which disputed the normative implications of Locke's mode of characterizing the master's appropriation of another's labor as equivalent to the smallholder's own (Hampsher-Monk 1976, 420–2).

From the democratic standpoint, then, capitalist property represents the most developed, or pure, form of private rule over people's material lives (Wood 1995, 19–48). The estate was fully severed from the state, consolidating the separation between private and public spheres of power. On the one hand, capitalist ownership dispensed with all paternalistic obligations to safeguard subjects' welfare, transferring them to the public domain. On the other, rule over the human interchange with nature was effectively privatized, protected in an "economic" domain beyond the reach of public deliberation and decision making. Ordinary producers were emancipated from the duties of deference that bound them to benevolent lords; but they became the subjects of a purely despotic enterprise, in the Aristotelian sense that owners command their services in the owners' private interest. Capitalist ownership surrendered the legitimate use of power—to tax, adjudicate, conscript—to the state. At the same time, the power of property reached deeper into material life itself. In contrast to precapitalist rulers, who fulfilled little entrepreneurial function themselves, capitalist owners assumed exclusive authority over production. Released from its general social duties, capitalist economic rule is no longer legitimated politically, by reference to the performance of communal functions. Private power is stabilized to the extent that democratic principles of rule are successfully insulated within a public domain.[12]

Capitalism and the Human Good

Communitarian critiques locate the barriers to human flourishing in the disembedded marketplace. The classical liberal claim that the market is conducive to individual self-realization is based on the wide field of choices it makes available. In challenging this claim, communitarian approaches focus on the ways in which markets predetermine this field of choice. As Polanyi observes, the reduction of labor to an article of commerce, bought and sold like other commodities, exposes working people to all the vicissitudes of the marketplace. Modern workers are indeed free to choose their economic activities, but their choices are subordinated to the requirements of capitalist accumulation. Divorced from the means of production, wage laborers are presented with an unacceptable exit option—a choice between the labor market and unemployment, with its attendant loss of status and livelihood.

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[12] See Friedman 1962, 23–4; Hayek 1944, 69–70. In their view, the stability of both democracy and capitalist ownership depends upon removing questions of economic rule—such as workplace organization and investment—from the province of public discourse.

In the view of democratic theorists, the pernicious consequences Polanyi attributes to market systems—insecurity, inequality, unfreedom, alienation—are more correctly attributable to private property regimes (Lindblom 1977, 105). The normative implications of markets are essentially by-products of the property relations within which they develop. This thesis is fleshed out in Thompson's accounts of early modern England, which serve as case studies of an emancipatory symbiosis of property and markets.[13] This period, roughly from the fifteenth to the eighteenth century, has been characterized as the golden age of the petty producer (Marx 1973, 510; Postan 1972, 142; Thompson 1968, 302, 338). Rebellious smallholders successfully loosened "traditional manorial, parochial, corporate and paternal controls" (Thompson 1991, 9; see also Hilton 1975, 18–9). Theirs was an economy of relatively untrammeled simple commodity production, a petty bourgeois world of small farmers and cottagers, masters and journeymen. Their economic independence was predicated on three conditions. First, underlying it was the expansion of market relations: The nonmonetary favors and services subjects customarily rendered their masters gave way to wages paid to free, mobile labor. Second, trade and industry expanded, not by way of concentration into large manufactures, but "on the basis of the multiplication of many small units of production, with much by-employment (especially spinning) coincident with many continuing forms of petty land tenure (or common right) and many casual demands for manual labor" (Thompson 1991, 41). Third, a large proportion of petty producers retained ownership of their means of production, giving them direct control of their immediate relations and terms of work. The eighteenth-century English crowd, writes Thompson (1991, 38–42), carried through

> a qualitative change in labor relations.... A substantial proportion of the labor force actually became more free from discipline in their daily work, more free to choose between employers and between work and leisure, less situated in a position of dependence in their whole way of life, than they had been before or than they were to be in the first decades of the discipline of the factory and of the clock.... Working often in their own cottages, owning or hiring their own tools, usually working for small employers, frequently working irregular hours and at more than one job, they had escaped from the social controls of the manorial village and were not yet subject to the discipline of factory labor.... Free labor had brought with it a weakening of the old means of social discipline.

Rather than undermine the independence of small producers, evolving markets fostered it. To be sure, their economy was never far above subsistence. But most were able to adjust to changes in the market, lengthening or shortening the working day, taking up subsidiary by-employments in their cottage, supplementing their subsistence from the common, the harvest, and occasional wage labor. Work retained the rich variety and irregular rhythms typical of preindustrial occupations. Gentry and master manufacturers were obsessed with the problems of work discipline; "mere wage-payment could never secure 'zealous services' " (Thompson 1968, 397). "The most characteristic complaint [among employers] throughout the greater part of the century was as to the indiscipline of working people, their irregularity of employment, their lack of economic dependency and their social insubordination" (Thompson 1991, 37).

The colonial encounter between capitalist and premodern economies repeated the clash over work discipline. "It proved necessary," observes Sahlins (1972, 86), "to beat [premodern] people into shouldering the white man's burden." Prices and wages merely produced "irrational" responses: "Recruited as plantation hands, they frequently showed themselves unwilling to work steadily. Induced to raise a cash crop, they would not react 'appropriately' to market changes." Work ceased when earnings satisfied customary requirements, regardless of the available opportunities, provoking charges of congenital indolence from colonial authorities and employers. Production for exchange alone did not succeed in reorienting the smallholding economy toward accumulation. Production for capitalist masters did, but this eventuality presupposed the transformation of the premodern property regime. The colonial period recapitulated the European transition to capitalism (Marx 1990, 931–40; Scott 1976, 7–8; Thompson 1991, 164–75). The encroachment of Locke's view of property "eliminated many of the possibilities for village poor to remain independent.... Increasingly, the only route to survival within the village led to a permanent dependence on those who hired labor. For many peasants it represented a decisive break with the past as the means of their subsistence now passed entirely out of their hands" (Scott 1976, 64).

Communitarian critiques of the capitalist economy mistakenly equate the market in labor with the privately ruled workplace. Adjoined only in capitalist economies, they are analytically distinct (Lindblom 1977, 105). Very different forms of ownership and management of firms are fully compatible with the maintenance of markets in goods and services, including labor. Alternatives featured within the democratic school involve worker self-management combined with various forms of public, cooperative, and small-scale private ownership (Nove 1991, 210–49; Schweickart 1996, 60–77). In this context, the relationship among firms and individuals is governed by the market. But employees enter the firm as voting members rather than as human capital, a change full of consequence for the determination of wages and working conditions, the distribution of work and leisure, and patterns of social conflict.[14]

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[13] Thompson's work is often represented as a critique of modern markets. The focus of his studies, however, is the impingement of capitalist property on the premodern economy: see "Custom, Law, and Common Right" (1991, chapter 3) and "Time, Work-Discipline, and Industrial Capitalism" (1991, chapter 6); see also Thompson 1968, Part Two. For his analyses of the marketplace, see "The Moral Economy of the English Crowd in the Eighteenth Century" and "The Moral Economy Reviewed" (1991, chapters 4, 5). But Thompson limits these investigations exclusively to "the market as dispenser of subsistence in time of dearth" (1991, 272). The bearing of the market on people's lives is in his view a product of class conflict (1991, 287). Like Hilton (1975), Thompson accentuates the emancipatory aspects of the petty bourgeois world that emerged in early modern England.

In refocusing normative critique on the workplace, democratic approaches redefine the nature of capitalist unfreedom, which may be best understood (borrowing from Hirschman 1970) as a loss of "voice" rather than restricted "exit." What modern wage laborers lack is the freedom to shape their conditions of work. A decision to quit is a grossly imperfect way of expressing dissatisfaction: Choosing one's mode of work is reduced to choosing whether to work at all (Lindblom 1977, 331). Capitalist ownership, not the labor market, presents modern wage laborers with a sharply constrained option set—between unemployment and authoritarian workplaces (Lindblom 1977, 45–51). In no other economic system is work so strictly supervised or so rigidly disciplined, and in no other system is the organization of work so immediately and directly responsive to the requirements of private accumulation.

Production for Profit

In the communitarian account, the modern market institutionalizes the imperative to accumulate. Its dictates regulate not only exchange relations but also social relations in general, including the organization of work. Capitalist owners and their managers are merely responding to the demands of competition, which oblige them to engage in an endless pursuit of profit. Democratic approaches challenge this conclusion. Market forces do not uniquely determine a firm's policies:

> There is no one obvious production and sales strategy that will avoid loss and maximize profits, sales, or whatever other desideratum the corporate leadership pursues. Even simple profit maximization would be complicated.... The fact is ... that even under highly competitive conditions, corporate executives cannot ... unerringly find one correct solution to their complex problems. Since they cannot, they have to exercise discretion. Even in principle there is no one least-cost solution to a complex problem (Lindblom 1977, 153, 155).

The relationship between marketplace and workplace is indirect. Samuelson (1957, 894) suggests that competitive markets generate the same imperatives whether workers hire capital or capital hires workers. Who disciplines whom, however, has significant consequences for the organization of the workplace (Pagano 1985, 134–6, 149–77; Schor 1991, 59–82, 126–38; Schweickart 1996, 224–36). Markets require firms to improve productivity by increasing output from the same inputs, one of which is labor. But costs and benefits—in income, leisure, and meaningful work—can be variously distributed. Capitalist firms measure labor costs exclusively in terms of the wages bill, independently of the preferences of workers for their own patterns of work. What workers sell, and owners buy, is not a concrete service or product but the right to command their capacity to work, subject to the terms of the contract. Revenues confirm managerial decisions; but owners do not bear the consequences of their choices on job satisfaction. Capitalist firms are thus characterized by a pervasive conflict between workers and employers: While workers have an interest in organizing and scheduling work to improve their well-being, the main interest of employers is to use labor to maximize the firm's profitability.

The ability of owners to establish control over the enterprise, to deploy employees among alternative tasks, is an essential condition of raising labor's output relative to its costs. This aim presents itself to owners as the problem of management. Its solution consists in reproducing the premodern distinction between ruling and working inside the workplace. Owners of early modern factories produced detailed legal codes, including penalties for infractions, to govern and regulate the length and pace of work (Thompson 1991, 383–90). They were only partially successful:

> Workers who are controlled only by general orders and discipline are not adequately controlled, because they retain their grip on the actual processes of labor. So long as they control the labor process itself, they will thwart efforts to realize to the full the potential inherent in their labor power. To change this situation, control over the labor process must pass into the hands of management, not only in a formal sense but by the control and dictation of each step of the process, including its mode of performance (Braverman 1974, 100).

The most significant advance in the assertion of rule within the workplace was accomplished by dissolving the premodern unity between the conception and execution of work. Smith's claim that the extreme specialization of the capitalist factory is necessary to maximize productivity is not persuasive (Braverman 1974, 76–83; Marglin 1978, 16–20; Murphy 1993, 155–64; Pagano 1985, 8–18). His argument mistakenly equates the technical separation of tasks with the development of job-specific skills. Specialization improves workmanship; confining individuals to "a few very simple operations," as Smith (1937, 734) acknowledges, destroys it, so that the worker "becomes as stupid and ignorant as it is possible for a human creature to become." Productivity is gained by analyzing complex processes into discrete steps; but the division of tasks need not involve detailed specialization. Technologies can be designed and combined with different patterns of work in a number of equally efficient ways (Murphy 1993, 19–33). The process which eroded workers' autonomy and skills, assigning each worker to a discrete task, was the special product of capitalism. Its purpose was not to improve productivity. Early modern manufactories were not more efficient than other forms of industrial organization. They enabled owners to augment profits by destroying the craft as a process under the control of the producers and reconstituting it under their own control (Braverman 1974, 75–9).

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[14] Empirical findings indicate widespread job dissatisfaction among contemporary employees. Lane's (1991, 287) review of the literature concludes that "workers are more dissatisfied even if the objective features of work have not declined." The prevailing theme in employees' moral economy is resentment over the absence of autonomy in their work (see Special Task Force 1973, 17–23, 94–6; Lane 1991, 237–59, 283–8).

Capitalist rule was given its most comprehensive formulation by Frederick Taylor (1976a, b) in the form of the principles of "scientific management." In his view, the main obstacle to profitability was not technical inefficiency but what he called "systematic soldiering," or "loafing." Its root cause was not "natural laziness" but, rather, "a careful study on the part of the workmen of what they think will promote their best interests.... The greater part of systematic soldiering ... is done ... with the deliberate object of keeping their employers ignorant of how fast work can be done" (Taylor 1976a, 30–3).[15] Taylor's remedy required divorcing employees from all participation in conceiving and directing their work, confining them instead to carrying out detailed instructions. Management functions were to be consolidated in the owners' hands, so that knowing what to do and actual doing became separate and mutually exclusive functions (Taylor 1976a, 98–99; 1976b, 36–8). Effectively reduced to one of the factors of production, workers were subject to the full authority and discipline of their employer.

Many of Taylor's recommendations for enforcing discipline have been superseded (Bendix 1963, 319–31).[16] Retaining control, however, remains the underlying preoccupation of owners and their managers, grounded in the recognition that their interests are at odds with those of their employees.

> The principal consideration is ... the stability and continued success of the company, which are secured and judged by "sound management." Accordingly, there is an unequivocal order of authority in the company, which is communicated in handbooks prepared for employees. The management decides upon what constitutes fair pay, fair treatment, and the right job for each employee, and it provides the opportunities for ... advancement. Clearly, there is no suggestion here that orders will be executed only if employees give their tacit consent. The implication is rather that they be executed because management has unquestioned authority (Bendix 1963, 328).

Markets require firms to produce efficiently. But the profile of the modern firm—work schedules, income distribution, and the occasions for skill development, personal accomplishment, and the exercise of responsibility—would look very different if employees mapped their own production and sales strategies.[17] To be sure, cooperatively managed firms do not obviate the tension between the diverse objectives of work. Difficult compromises must be reached among individuals with different preferences and needs—for satisfactory work, leisure, and income. Unlike capitalist firms, however, in which owners apportion benefits and costs, the members of the cooperative themselves determine the trade-offs among these aims (Pagano 1985, 174–7; Schweickart 1996, 231–4). What is important is that these decisions are not the prerogative of ownership; they are properly political, involving the exercise of power, and hence subject to the democratic rule of workers themselves (Walzer 1983, 302–3).[18]

The Domain of Need

A central concern of communitarian critiques is the change occasioned by modern markets in the relationship between public and private provisioning. The domain of security and welfare contracted dramatically in the early history of capitalism, abandoning the distribution of staples to the vagaries of the marketplace. In defense of the new economic principles, Smith (1937, 14) chose his examples carefully: "It is not from the benevolence of the butcher, the brewer, or the baker, that we expect our dinner, but from their regard to their own interest. We address ourselves, not to their humanity but to their self-love, and never talk to them of our own necessities but of their advantages." In early modern Europe, bakers, even more than millers, "were considered as servants of the community, working not for a profit but for a fair allowance.... The baker's allowance or profit was calculated strictly according to the Assize of Bread, whereby either the price or the weight of the loaf was ordered in relation to the ruling price of wheat" (Thompson 1991, 194). Smith's doctrine overturned the premodern understanding of the right to subsistence, effectively relocating the domain of need within the purview of the marketplace: Necessities, foodstuffs in particular, became articles of commerce; just price was replaced by market price; and public service gave way to self-love. Polanyi (1968, 38–53) is correct to point out that nineteenth-century social movements were in part efforts to reassert society's governance over the distribution of the means of consumption (see also Mauss 1967, 65–8).

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[15] Taylor's concerns reflected the preoccupations of American employers not only with the intractability of workers but also, and equally important, with the growing tide of the trade-union movement (Bendix 1963, 274, 281–2).

[16] Employers did not readily accept Taylor's conception of their role, at least in part because they "regarded his methods as an unwarranted interference with managerial prerogatives." Nevertheless, "the principal ideas in Taylor's work were widely accepted: the social philosophy rather than the techniques of scientific management became part of the prevailing managerial ideology" (Bendix 1963, 280, 281).

[17] Abrogating the political entailments of property would dramatically reduce the inequalities produced by the free operation of the market. "The gross inequalities that we see around us today could not be sustained. They derive more significantly from status hierarchies, organizational structures, and power relations than from free markets" (Walzer 1983, 117). Scarcity, ambition, and talent would still command a premium. But "the more perfect the market, the smaller the inequalities of income will be, and the fewer the failures. If we assume a rough equality in mobility, information, and opportunities for training, it ought to be the case that the most attractive jobs will draw the most applicants, and so the wages they pay will fall; the less attractive jobs will be shunned, and so those wages will rise" (Walzer 1983, 116–7).

[18] Cooperatives permit considerable variation in the scope and level of workplace participation. Clearly, some balance needs to be struck between authority and accountability, conditioned by the nature and size of the firm (see Whyte and Whyte 1988, 209–16). Surveying twenty-five years of research, Blumberg (1969, 123) concludes "there is hardly a study in the entire literature which fails to demonstrate that satisfaction in work is enhanced or that other generally acknowledged beneficial consequences accrue from a genuine increase in workers' decision-making power." For more skeptical assessments of the claims made on behalf of workplace democracy, see Lane 1991, 329–36; Greenberg 1986, 113–4.

The theory of the disembedded economy, however, misconstrues the nature of the change. According to Thompson (1991, 201–2), Smith's theory entails "demoralizing" the economy, evacuating it of intrusive normative considerations. But the central dilemma is not the market's independence from society's governance, the cash nexus, or production for gain. Smith's doctrine raises a moral concern because it would strip the community of its authority to distribute the resources needed to guarantee a basic livelihood. What its axioms violate is the principle that subsistence goods, things which serve needs as a society understands them, should not be sold for profit (Thompson 1991, 272–4). This explains the moral roots of early cooperatives and friendly societies, indeed, of every demand for an expansion of social welfare—these are not attempts to abolish the market but, rather, to (re)establish its proper scope, to create a system of social insurance for the provision of certain key social goods.[19] The contest between democracy and the market lies here, in defining the nature and extent of needs. Once a definition is generally accepted, distribution is removed from the operation of the market and replaced by mutual provision. And once its boundaries are appropriately drawn, the marketplace, traditional and modern, ceases to present a moral dilemma (Walzer 1983, 107–8).

CONCLUSION

In Booth's view, the theory of the embedded economy engenders normative reflection on our interchange with nature. At the same time, however, he concludes that its moral vision is little more than "a museum piece," a warning against the dangers of economic romanticism (Booth 1994, 663). Indeed, reembedding the marketplace, subordinating production to serve a substantive way of life, incurs considerable moral costs. Leaving aside questions of efficiency, the concentration of political control required to plan complex relations of production and distribution constitutes a significant threat to liberty. Contemporary communist states bear an uncomfortable resemblance to premodern despotic households written large.

The democratic economic school also draws its normative vocabulary from the premodern world. Its source is not the embedded economy but the aspirations of petty producers for economic masterlessness. The practical aim of this school, then, is not the abolition of the marketplace but the democratization of property, the modern workplace in particular. The moral vision guiding it entails the re-creation of the precapitalist world of simple commodity production in modern form, to reproduce

> under the conditions of large-scale industry and agriculture, just those features of the petty-bourgeois economy that make its risks, and the inequalities that follow from those risks, defensible. Democratic decision making, like petty-bourgeois smallholding, is a way of bringing the market home, connecting its opportunities and dangers to the actual effort, initiative, and luck of individuals (and groups of individuals) (Walzer 1983, 118).

The task of the democratic critique is to realize the break which the transition from the traditional to the modern economy failed to make, to abrogate the rule of capitalist property.

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[19] Establishing a domain of security and welfare, in which goods and services are publicly provided, need not entail a turn to communitarian concerns with promoting a particular way of life. Democratic approaches suggest an alternative conception. Grounded in a distinction between substantive and formal self-determination, the purpose of restricting the operation of the market in light of socially defined needs is to enhance what Rawls calls "the worth of liberty," that is, to permit individuals to pursue their own chosen way of life, free from social and natural impediments over which they have little control (Rawls 1971, 204–5; Macpherson 1973, 57–76; Nove 1991, 222). Democratic theories (of the sort I defend) endorse a "neutral state," that is, one which does not deliberately rank different conceptions of the good; communitarian theories favor a "perfectionist" state, that is, a state that does publicly rank them. In Walzer's (1990) view, public policy should be guided by a modest perfectionism, designed to sustain cultural complexity.

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The Evolutionary Stability of Cooperation

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Is cooperation without central authority stable? If so, how robust is it? Despite what might be the conventional wisdom, The Evolution of Cooperation *did not solve this problem deductively. In fact, results obtained later by others seem to have contradicted the book's main message. Reexamining this exceptionally influential work yields a new picture. Part of Axelrod's evolutionary story turns out to be false. But the main intuition, that retaliatory strategies of conditional cooperation are somehow advantaged, proves correct in one specific and significant sense: Under a standard evolutionary dynamic these strategies require the minimal frequency to stabilize. Hence, they support the most robust evolutionary equilibrium: the easiest to reach and retain. Moreover, the less efficient a strategy, the larger is its minimal stabilizing frequency; Hobbesian strategies of pure defection are the least robust. Our main theorems hold for a large* class *of games that pose diverse cooperation problems: prisoner's dilemma, chicken, stag hunt, and many others.*

It has been ably and amply argued that cooperation is vital for systems without central authority (Axelrod 1981; Milgrom, North, and Weingast 1990; Oye 1986). Even formal institutions rarely specify everything by role or employment contract; hence informal cooperation, when viable, is the glue of such systems (Chisholm 1989, Heclo 1977, Matthews 1960, Shils and Janowitz 1948). Yet, what do we really know about the evolution of cooperation? In some ways we know less than we think: Much of what is commonly believed is not true. Many probably believe that Axelrod's seminal work, *The Evolution of Cooperation* (1984), established the merits of the simple reciprocity-based strategy of Tit for Tat (TFT). Hence we know that TFT "is superior to all other [strategies] in playing repeated games of prisoner's dilemma" (Trivers 1985, 391). Hence we know that TFT is evolutionarily stable and is successful because it is nice (never the first to defect), retaliatory (it will defect in period $t + 1$ if its partner defects in t), and forgiving (it will return to cooperation in $t + 1$ if its partner cooperated in t). Hence we know that TFT gets the highest score, if the future matters sufficiently and enough others play TFT.

In fact, we know none of these things, because the above claims are either *false or ambiguous*—the seemingly settled facts about the evolution of cooperation are not settled at all. Indeed, the most important part of the conventional wisdom, that TFT is evolutionarily stable, has been contradicted by a *Nature* article with the unequivocal title of "No Pure Strategy Is Evolutionarily Stable in the Repeated Prisoner's Dilemma" (Boyd and Lorberbaum 1987). So in what sense, if any, is TFT stable? Do cooperative strategies enjoy an evolutionary advantage over less cooperative ones? If so, what is the difference? The stability of cooperation is an issue of the foremost importance for the science of politics. Yet, many readers will be surprised, we believe, to find out how little we know about it.

And so our task is twofold: First we must show that there is a problem and then we must solve it. We begin by analyzing the claims and counterclaims. The inherent logic of this inquiry generates a succession of auxiliary findings leading to our main results, which are presented near the end of the article. Since the path that leads to them is essential to understand their meaning, comprehension requires patience.

The article is organized as follows. First we set out assumptions and give a brief overview of evolutionary game theory. We then analyze the fundamental question of whether cooperative strategies like TFT are, indeed, evolutionarily stable. In the following section we examine why and in what ways TFT's crucial properties are evolutionarily important. Next, we generalize the problem of cooperation from the prisoner's dilemma to a much more general class of games. We then deliver the fundamental results, theorems 7–9, for this class. Theorem 7 shows that conditionally cooperative strategies are, indeed, evolutionarily advantaged: Across all ecologies with a finite number of strategies evolving under the standard replicator dynamic, these strategies require the minimal populational frequency for stability. In this sense they support the most robust evolutionary equilibrium. Theorem 8 demonstrates that *only* strategies which are sufficiently cooperative and provocable can support this most robust equilibrium. At the other end of the spectrum, theorem 9 and its corollary show that Hobbesian strategies—those that always defect with their clones—are the most fragile of all evolutionarily stable strategies. These results are established for a large class of games that involve many different kinds of cooperation problems

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(e.g., some coordination games, chicken, stag hunt, and others).

EVOLUTIONARY ANALYSIS: BASIC CONCEPTS AND DEFINITIONS

Before diving into the heart of the issue, we must emphasize one point about our assumptions. The evolution of cooperation has been intensively studied over the last decade.[1] Since this scholarship is not unified and lacks a single central result that can serve as a reference point, knowing what happens under the standard set of assumptions is important. Our results follow from these standard premises. *These assumptions are as postulated by Axelrod and other participants in the debate* (Axelrod 1981, 1984; Axelrod and Hamilton 1981; Boyd and Lorberbaum 1987; Farrell and Ware 1989).

Specifically, we model the problem of cooperation by a tournament structure (Axelrod 1984). Here, people meet randomly to play a two-person prisoner's dilemma (PD). The PD is repeated: With a fixed probability, δ, two players will encounter each other again in the next round ($0 < \delta < 1$). The standard PD assumptions govern the one-shot game: Each player can either cooperate or defect, with resulting payoffs T, R, P, S such that $T > R > P > S$, and $2R > T + S$. (These are standard labels: T is the temptation payoff to the player who defects while his or her partner cooperates, R is the reward for mutual cooperation, P is the punishment for mutual defection, and S is the sucker's payoff to the player who cooperates while his or her partner defects.) These choices and payoffs hold in every round. At the start of a new period each player knows what his or her partner did in the previous rounds. The "dilemma" is that in a single-period game, defection is a dominant strategy—player 1 is better off defecting no matter what player 2 does—but if both use their dominant strategies, then both are worse off than if they had cooperated.

Since cooperation is strongly dominated in the one-period game, the only equilibrium outcome is for both players to defect. The same is true when the game is iterated but δ is low (the future benefits from mutual cooperation have a lower expected value than the benefits from defecting on the current move), so defecting always remains the best response to all strategies. Yet, if δ is large enough, then the expected payoff from future cooperation suffices to support constant cooperation in Nash equilibrium; this is just one of a great variety of equilibria that are possible with sufficiently large δ. In fact, standard folk theorems state that for sufficiently high δ, any "amount" of mutual cooperation (from 0 to 100%) can be sustained in Nash equilibrium.

Since the repeated game is strategically identical to the one-shot if δ is low (the only equilibrium being mutual defection), it makes sense to study the evolution of cooperation only when δ is "sufficiently" big. Hence, all our results assume that the "shadow of the future" is sufficiently long.

Definitions of Strategies

In addition to TFT, we use several other strategies as examples. (Recall that TFT cooperates in period one and thereafter cooperates if its opponent cooperated in the previous period, and defects otherwise.) For easy reference, we now list full names, abbreviated names, and definitions of the most commonly used strategies. The text will use the abbreviations.

TIT FOR 2 TATS (TF2T): Cooperate in periods 1 and 2. Thereafter, defect in any period $k \geq 3$ if and only if your opponent defected in $k - 1$ and $k - 2$.

SUSPICIOUS TIT FOR TAT (STFT): Defect in period 1, and for any period $k \geq 2$ do what your opponent did in $k - 1$.

ALWAYS DEFECT (ALL D): Defect unconditionally in all periods.

ALWAYS COOPERATE (ALL C): Cooperate unconditionally in all periods.

GRIM TRIGGER (GT): Cooperate in period 1, and never defect first; if the opponent defects in any period k, defect from $k + 1$ on.

Evolutionary Game Theory

All evolutionary analyses are based on the principle of survival of the fittest. What survive or die in evolutionary games are strategies. Thus, as Axelrod remarked in his characteristically succinct way, "the evolutionary approach is based on a simple principle: whatever is successful is likely to appear more often in the future" (1984, 169). This is a general idea. A precise definition requires a careful setup. Let an ecology or population be a *finite* set of strategies, $j_1, \ldots, j_N$, and their respective proportions $p_1, \ldots, p_N$, with $\Sigma p_i = 1$. (For example, Axelrod's first tournament had 15 strategies; each was one-fifteenth of the ecology.) Let $V(j_k)$ denote a standardized tournament score of strategy j_k. $V(j_k)$—which corresponds to the "fitness" of j_k—is defined as the standardized sum of j_k's payoffs across all its pairwise games with other strategies in the population: $V(j_k) = p_1 V(j_k, j_1) + \ldots + p_N V(j_k, j_N)$. ($V(i, j)$ denotes i's payoff when i plays j.) The assumption that the number of strategies in the population is fixed and finite is important. It permits the analysis of games with infinitely many pure strategies, such as infinitely repeated games, as well as games in which both pure and mixed strategies replicate. What we have to keep in mind, however, is that every evolutionary game must be interpreted as *population-specific*: The process unfolds in a space defined by a specific set of strategies. (For a more extensive discussion see Bendor and Swistak 1996c.)

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[1] For a review of this work see Axelrod and Dion (1988) and Axelrod and D'Ambrosio (1994). Other relevant articles on evolutionary games are summarized in our last section. Much of the recent work investigates stability under "trembles" or other stochastic conditions that alter the assumptions of the original debate on the evolution of cooperation. Naturally, different assumptions describe different strategic ecologies; which set best describes a particular ecology is an empirical question that cannot be decided a priori.

We will call a dynamic an *evolutionary process* if for all strategies j_l and j_k the following conditions are satisfied:

$$\frac{\Delta p_k}{p_k} < \frac{\Delta p_l}{p_l} \text{ iff } V(j_k) < V(j_l) \tag{1}$$

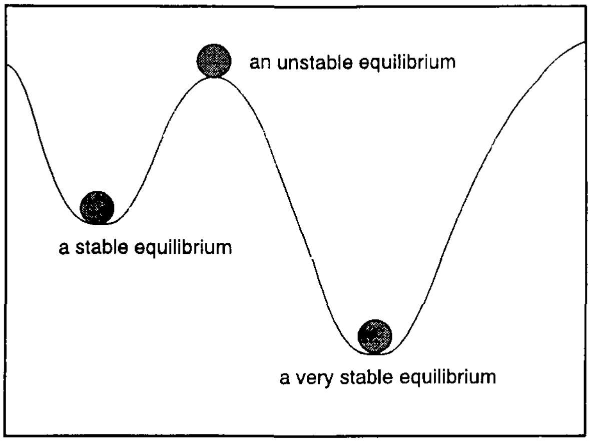
$$\frac{\Delta p_k}{p_k} = \frac{\Delta p_l}{p_l} \text{ iff } V(j_k) = V(j_l) \tag{2}$$

where the V's denote tournament scores in generation T, and assuming that p and p' denote proportions in two consecutive generations T and $T + 1$, Δ refers to $p' - p$. When changes per generation are sufficiently small, a process that is discrete in time (i.e., time points are generations 1, 2, 3, . . .) can be approximated by a process that is a continuous function of time. Conditions 1 and 2 with dp/dt's substituted for Δp's define the continuous form of an evolutionary process. Condition 1 represents the basic monotonicity postulate that more fit strategies have higher relative growth rates. Condition 2, together with 1, implies that the process is in equilibrium only if all strategies in an ecology are equally fit. (What we call here an evolutionary process is often referred to as a monotone process [cf. Bendor and Swistak 1996c; Friedman 1991, 1994; Nachbar 1990].)

To summarize: The heart of evolutionary game theory is the basic dynamic postulate that the better a strategy performs in the present, the faster it grows. The three elements of an evolutionary game—a strategy's stability, fitness, and pairwise payoffs against other strategies—are all related. This obvious though important point turns out to be very consequential.[2]

With these concepts defined, we can move on to the notion of evolutionary stability. In general, a system is dynamically stable if, after being perturbed slightly, it returns to its original state. To visualize this, imagine several hills separated by valleys, as in Figure 1. A ball perched on a hill is in equilibrium: As long as no force acts on the ball, it will stay where it is. But this is not a stable equilibrium, for a slight push will send the ball into a valley. Once the ball is in a valley it is stable: A small disturbance will move it only part way up a hill, and it will then roll back down into the valley. Intuitively, the deeper the valley, the more stable is the equilibrium, for deeper valleys require larger shocks if the ball is to be dislodged. The deeper valleys represent the more robust equilibria. This notion of robustness will be crucial for our main result, the Characterization Theorem.

FIGURE 1. The Robustness of Equilibria



When the system is a population of strategies, the original position is the ecology's initial composition of strategies; a perturbation is a change in this composition. By condition 2, in an evolutionary equilibrium all strategies are equally "fit." Suppose, then, that strategy i has completely taken over an ecology. Obviously, the system is then in equilibrium. We can analyze its stability by subjecting it to small shocks—introducing new strategies—and seeing whether the ecology returns to the original situation wherein everyone played i.[3]

TFT AND THE IDEA OF EVOLUTIONARY STABILITY

In what *sense* are TFT and other conditional cooperators evolutionarily stable? As we shall see, different scholars have perhaps unknowingly had different concepts in mind when answering this question. Clarifying related conceptual issues is a major task of this section.

The shortest path to this goal happens to lead through the back door, via the notion of disequilibrium. Just as different equilibria can be more or less robust, so can disequilibria be more or less destructive. Consider the most fatal disequilibrium of all.

Fundamental Instability

Suppose that all decision makers use the saintly ALL C. Although, by condition 2, the process is in equilibrium, the balance is utterly unstable: If even one person were to switch to ALL D, then s/he would do much better than the "native" ALL C. No matter how small the invasion and no matter what evolutionary process governs reproduction, ALL C will be driven to extinction. It is the paradigmatic example of what we call a *fundamentally unstable* strategy, which we define as follows. Suppose that $1 - \varepsilon$ of all players in an ecology

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[2] Indeed, some readers of Maynard Smith have, despite his warnings (1982, 204), come to regard the evolutionary stability of a strategy i as equivalent to i satisfying the following pairwise payoff condition for all $j \neq i$,

$$V(i, i) \geq V(j, i), \text{ and if } V(i, i) = V(j, i), \text{ then } V(i, j) > V(j, j) \tag{3}$$

Doing so is a mistake: while (3) implies stability under some conditions, it does not imply it under others (Bendor and Swistak 1992, 1996c; Weibull 1995).

[3] In this standard kind of dynamic analysis, one perturbs the system and then lets it reach a new equilibrium (e.g., the ball comes to a new resting place). Clearly, one may obtain different results if the system is shocked repeatedly, without allowing it to reach equilibria in between disturbances (cf. Young and Foster 1991).

FIGURE 2. Problems with the Collective Stability Concept

Game A

	x	y
x	6, 6	0, 6
y	6, 0	3, 3

Game B

	x	y
x	3, 3	0, 3
y	3, 0	6, 6

Note: In these two one-shot games the pure strategy x is collectively stable (i.e., $V(x, x) \geq V(j, x)$ for all j) yet fundamentally unstable. With $(1 - \varepsilon)$ x's and ε y's in Game A $V(x) = 6(1 - \varepsilon)$ whereas $V(y) = 6(1 - \varepsilon) + 3\varepsilon$; in Game B $V(x) = 3(1 - \varepsilon)$ whereas $V(y) = 3(1 - \varepsilon) + 6\varepsilon$. So $V(x) < V(y)$, for all values of ε. Game A shows how a dynamic can go from the efficient state of (x, x) to the inefficient (y, y); Game B shows the opposite. Together they show the problems with collective stability.

play the native strategy i, while a few ($\varepsilon > 0$) play a mutant strategy j. Furthermore, assume that j beats i ($V(j) > V(i)$) for every value of ε. Then, i's frequency will fall across generations until i disappears, no matter which evolutionary dynamic holds sway or how small was the invasion. Such an i we call fundamentally unstable.

The Necessary Condition for Stability

ALL C's problem is that it is not provocable and so cannot defend itself against exploitation. In more general terms, ALL C is not a best reply to itself—a failing that makes it fundamentally unstable (Bendor and Swistak 1992, 1996c).[4] STFT, for instance, suffers from the same problem. Because STFT defects in all periods when playing its clone, yet can cooperate indefinitely with a more "patient" strategy, say, TF2T, it is not the best reply to itself if δ is large enough. Any invasion by TF2T will wipe out the population of STFT, which is fundamentally unstable.

If a strategy fails to be a best response to itself, the dynamic effects are critical—not just in the iterated prisoner's dilemma (IPD), but in all games. Hence, it is good to introduce this condition formally. A strategy i is a best reply to itself if for all strategies j,

$$V(i, i) \geq V(j, i). \qquad \textbf{(4)}$$

This, of course, is better known as a pair of strategies (i, i) being in Nash equilibrium.

The Instability of "Collective Stability"

Axelrod referred to a strategy satisfying condition 4 as collectively stable (1981, 310). While, as we have seen, condition 4 is necessary for evolutionary stability, it is *not* sufficient: Strategies that satisfy condition 4 may be fundamentally unstable. (See Figure 2 for two examples.) For this reason they should not be called (collectively) stable; condition 4 does not imply, in general, any dynamic stability (Boyd and Lorberbaum 1987, Sugden 1988, Swistak 1989). Put simply, Axelrod's stability concept was incorrect.

Does TFT pass the necessary test of condition 4? TFT is, in fact, a best reply to itself if δ is sufficiently big (Axelrod and Hamilton 1981). The formal proof is simple and agrees with an intuition that TFT's provocability and niceness together ensure that for large δ, the best reply to TFT is constant cooperation. In particular, the kind of invasion that overcame STFT cannot destabilize TFT; nor can ALL D destroy a population of TFT as it did ALL C.

No Strategies are Strongly Stable

Yet, TFT cooperates with many different strategies. This causes problems for TFT's evolutionary stability (Selten and Hammerstein 1984). Suppose in a population in which everyone plays TFT someone switches to TF2T. What will happen? Since both TFT and TF2T are nice, everyone in this ecology will cooperate in every period and TFT and TF2T will be equally fit. Hence, the ecology will *not* return to its original state of everyone playing TFT—the invading strategy will endure under any dynamics. And this is not an arcane technical property; it has a natural and important empirical interpretation. When everyone is playing TFT, everyone cooperates in every period. Hence, TFT's retaliatory nature remains latent. And because an ability that is never deployed may deteriorate, some actors may start using softer strategies, such as TF2T. Thus, there may be an invasion of TF2Ts. As long as only nice strategies are in the ecology, however, this invasion is inconsequential: learning cannot occur and the "mutant" TF2T cannot be driven out. Indeed, the mutant cannot even be *detected*; TF2T is a "neutral mutant" of TFT, being behaviorally indistinguishable from TFT if no "non-nice" strategies are in the population. In general, we will say that j is a *neutral mutant* of i if they are behaviorally identical when only i and j are in the ecology.[5]

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[4] In other words, two ALL Cs are not in a Nash equilibrium. For related results, see Nachbar 1990 and Hirshleifer and Martinez Coll 1988 (379).

Hence, as Selten and Hammerstein conclude, TFT cannot be evolutionarily stable in a strong sense, for it cannot eliminate neutral invaders and therefore cannot restore the original equilibrium. The same holds for *all* pure strategies in the IPD. Suppose the native is STFT. This native can be invaded by ALL D, a neutral mutant of STFT. Similarly, ALL D is vulnerable to an invasion by STFT.[6]

Yet, in these examples, though the native strategy is unable to drive the mutant out, the mutant will not (absent other mutations) spread in the population. This suggests a distinction between two types of evolutionary stability: a native strategy is *strongly* stable if, after it is invaded, its proportion increases; a strategy is *weakly* stable if, after it is invaded, its proportion does not further decrease. (The Appendix defines these concepts formally. An extended discussion of these ideas is given in Bendor and Swistak 1996c.) Thus, TFT may yet be weakly stable, although we now know that it is not strongly stable.[7]

Indeed, the concept of strong stability is generally inapplicable to repeated games. The reason is that neutral mutants exist for every pure strategy in all repeated games. (The proof is easy.) And even in one-shot games, neutral mutants exist whenever the game has multiple stages (e.g., buying guns or butter, followed by a decision whether to go to war). So for pure strategies, the criterion of strong stability must be restricted to one-period games with a single stage. These are games of little substantive importance in political science.

No Strategies Are Even Weakly Stable under All Processes

In one sense, that TFT is not strongly stable seems to have little bearing on the evolution of cooperation. Although TF2T can invade TFT, the ensuing evolutionary equilibrium is behaviorally identical to the preinvasion state of universal cooperation. But this conclusion may be premature, for now we must examine the stability of this new equilibrium. What happens if there is another slight perturbation? Suppose a STFT invades so that the ecology contains a small fraction of TF2T's and STFTs among the native TFTs. This invasion is more substantial. Since different strategies have different fitnesses, selection mechanisms have a chance to work. How will such an ecology evolve?

The strategies do not do equally well. TFT continues to do well with TF2T and vice versa. But TFT's provocability gets it into trouble with STFT, for following STFT's defection, TFT retaliates in period two, which STFT then punishes, and so on, indefinitely. TF2T avoids this vendetta with STFT. Because TF2T forgives isolated defections, it cooperates in period two, which ensures mutual cooperation with STFT from period three onward. Since $2R > T + S$, TF2T's ability to elicit cooperation from STFT is superior to a vendetta.[8] Consequently, TF2T's fitness exceeds TFT's, which exceeds STFT's.

What are the dynamic implications of TFT not being maximally fit? Will the population of TFT become destabilized (Boyd and Lorberbaum 1987)? Not necessarily. What happens depends on which *specific* dynamic governs replication.

TFT can be destabilized by certain evolutionary processes. Suppose, for example, that actors simply switch to the strategy with the highest fitness.[9] Then, after STFT's invasion, the decision makers will adopt TF2T.[10] Hence, TFT would not be even weakly stable under this "imitate-the-winner" process. (In general, we will call an evolutionary process *imitate-the-winner* if only maximally fit strategies increase under it.) Furthermore, in the new status quo everyone plays TF2T, a fragile equilibrium. TF2T's willingness to forgive isolated defections invites exploitation by, for example, ALTERNATOR, which defects in odd periods and cooperates in even ones. For any value of δ, an arbitrarily small invasion of ALTERNATOR can drive TF2T to extinction. In turn, ALTERNATOR can be wiped out by an arbitrarily small invasion of ALL D. Thus, the entire structure of cooperation can collapse.

The invadability of TFT and the subsequent erosion of cooperation may occasion some surprise. Is not TFT a best reply to itself? Does that not imply that TFT is maximally fit, if sufficiently common in the ecology? The answer to the first question is "yes," but as we have just seen, the answer to the second is "no." For convenience, we will say that a strategy which is maximally fit in any ecology in which it is sufficiently common is *unbeatable*. The difference between a best-reply strategy and an unbeatable one turns on ecological effects. A strategy being a best response to itself is a pairwise property. Being unbeatable in a tournament involves more complex ecological effects, depending not only on how the native i does against j, but also on how each fares against a third strategy k. Boyd and Lorberbaum (1987) use this observation to prove that *no* pure strategy is unbeatable in the IPD.[11] Here is their argument. Take any pure native i. If a neutral mutant, i', invades, then $V(i, i) = V(i', i) = V(i, i') = V(i', i')$. Accordingly, if another mutant j appears, the ranking of i's and i''s tournament scores will depend on how well they do against j, that is, on $V(i, j)$ versus $V(i', j)$. Boyd and Lorberbaum show that for every pure native i, there always exist a neutral mutant i' and a third strategy j that will favor i' over the native i, that is, $V(i', j) > V(i, j)$. Hence, no pure strategy in the IPD is unbeatable.

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[5] More precisely, for any strategy i, j is a *neutral mutant* of i if in an iterated game with i, i and j follow the same path as i does when playing another i; nodes that distinguish i from j are never reached.

[6] It is precisely for this reason that no pure strategy in the IPD can satisfy Maynard Smith's payoff condition (3).

[7] For processes that satisfy certain regularity conditions—e.g., continuous in p's with continuous partial derivatives, or Lipschitz continuous (cf. Weibull 1995)—weak stability implies *stability* (or *Lyapunov stability*) and strong stability implies *asymptotic stability*. It is important to note, however, that in general our concepts of weak and strong stability differ from stability and asymptotic stability. Others have used different names for weak stability: Selten (1983), Sobel (1993) and Warneryd (1993). Bomze and van Damme (1992) use the same name as we do.

[8] More precisely, if δ is big enough then the best response to STFT is always to cooperate.

[9] Axelrod (1984, 159) used a variant of this rule in his analysis of territorial dynamics: in generation $T + 1$ a decision maker adopts whichever strategy scored highest in her "neighborhood" in T. Thus players imitate local winners.

[10] If everyone imitated the winner, then every strategy not maximally fit in T would become extinct in $T + 1$. Hence, all such strategies would have equal growth rates, despite (possibly) unequal fitnesses, and so the process would fail condition 1. To accommodate this we may require, for instance, that a person using strategy j does not change his strategy with probability $p(V(j))$, where $p(\cdot)$ is strictly increasing in $V(j)$. (For example, if the maximal fitness in T is V^*, then $p(V(j)) = \varepsilon(V(j)/V^*)$, with $0 < \varepsilon \leq 1$.) If a person does change his strategy, he adopts the winning one.

This result generalizes to all substantively interesting games, as shown by theorem 1. (We have proven this result elsewhere—Bendor and Swistak 1992, 1996c.)

THEOREM 1: *For sufficiently high* δ, *no pure strategy is unbeatable in any repeated nontrivial game.*

We say that a symmetric one-shot game is trivial if each player has a strategy that gives him the biggest payoff in the game, no matter what action the opponent takes. Such games exhibit no strategic interdependence whatsoever.

Note that if a strategy is beatable, then it can be destabilized under imitate-the-winner dynamics. As theorem 1 shows, this is true of all pure strategies in virtually all repeated games.

But Stability under Some Processes Is Still Possible

That TFT can be destabilized by imitate-the-winner does not imply that it can be destabilized by *all* evolutionary processes. Imitate-the-winner is only one type of dynamics that satisfy the basic evolutionary postulate. While imitate-the-winner processes are intuitively compelling, other dynamics also may have sound behavioral interpretations.

A specific dynamic, adopted by Axelrod in the "ecological analysis" of his tournaments, presumes growth that is linearly proportional in fitness. If p_k and p'_k denote strategy j_k's proportions in two consecutive generations, then the discrete form of the process can be formally defined as

$$p'_k = p_k \frac{V(j_k)}{\bar{V}} \qquad (5)$$

where $\bar{V}$ is the population's average fitness in the current generation. The continuous form of the PFR is defined and discussed in Bendor and Swistak (1996c), for example. This evolutionary process, both discrete and continuous, is sometimes called the *proportional fitness rule* (PFR), a term we also shall use. It is important to note that $V(j_k)/\bar{V}$ is larger than one if j_k's fitness, $V(j_k)$, exceeds $\bar{V}$, the average fitness in the population. So, under the PFR, j_k's relative frequency will keep increasing as long as its fitness is higher than average.

Consequently, TFT need not be unbeatable to flourish under the PFR. Under what conditions will TFT thrive under this growth rule? Before we address this open question, we must examine the nature of the PFR.

The PFR—more often called the "replicator dynamics"—is almost universally used in evolutionary models of social phenomena.[12] The rule, however, has been mechanically transplanted from biology, where it has an important substantive interpretation,[13] to the social sciences, where it does not. Until recently no one has explained why the PFR should be considered a plausible mechanism of behavior transmission, though the need for some justification has been apparent (Friedman 1991, Mailath 1992). Below we propose a framework that provides a behavioral interpretation of the PFR and other evolutionary processes. (Cabrales [1993]; Gale, Binmore, and Samuelson [1995]; and Schlag [1994] have recently proposed alternative explanations of the PFR.)

Evolutionary Processes, PFR, and the Meaning of Evolutionary Change

A key notion of evolutionary analysis is that players tend to adopt successful strategies and discard weak ones. There are two obvious measures of success. The first is a strategy's current relative performance, $V(j_s)/V^*$: the strategy's raw fitness score, $V(j_s)$, compared to some fitness parameter of the population, V^*, such as mean population fitness or maximal fitness. This indicates how well j_s does in the current generation. The second measure, a strategy's proportion in the current generation (p_s), reflects j_s's performance in past generations: With players lacking intergenerational memory, this is the only available measure of j_s's past success. Formally, then, strategy j_t's propensity to switch to strategy j_s, $\pi_{j_t j_s}$, is a function, F, of j_s's proportion in the population, p_s, and j_s's relative performance, which we call $V^*(j_s)$. In short, $\pi_{j_t j_s} = F(p_s, V^*(j_s))$. Since p_s and $V^*(j_s)$ both measure j_s's success, past and present, it is natural to assume that F is nondecreasing in both variables. We now consider two other properties of F and their meaning for the type of actor they describe.

One substantively important consideration is the relative effects of p_s and $V^*(j_s)$ on F. If a change of Δ in frequency produces a larger change in F than the same change in relative performance (i.e., $F(x + \Delta, y) > F(x, y + \Delta)$), then this means that an individual's propensity to switch is influenced more by the past (measured by the current proportions) than by the current performance. Hence, at one extreme we have *homo sociologicus*, whose F is a function only of the past or tradition. At the other extreme we have *homo economicus*, whose F is a function only of strategies' current performance. At this extreme are processes like imitate-the-winner. Between the extremes is an actor who weights the past and present equally:

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[11] Boyd and Lorberbaum's (1987) result has been extended to finitely mixed strategies (Farrell and Ware 1989) and to completely mixed strategies (Lorberbaum 1994).

[12] We use PFR (a name occasionally used in biology) instead of the more conventional name "replicator dynamics," which we believe has misleading connotations outside biological contexts.

[13] In genetics the PFR is implied by the classical selection equation, which in turn follows from the Hardy-Weinberg law of population genetics (Hofbauer and Sigmund 1988, Schuster and Sigmund 1983).

$$F(x + \Delta, y) = F(x, y + \Delta). \qquad (6)$$

This property turns out to be an important characteristic of the PFR.

A second substantively important consideration turns on how the propensity $F(x, y)$ changes as a function of the two variables. Consider, for example, the first variable—the proportion. On the one hand, imagine a process for which a fixed increase of Δ in p_s would, other factors equal, increase F more the smaller the p_s (i.e., $\partial^2 F/\partial p_s^2 < 0$). This type of adjustment would be observed, for instance, with elite imitation processes, in which the smaller the elite, the higher is its status, hence the stronger its influence. (We define this process in detail in Bendor and Swistak 1996c. Many other processes are described in, e.g., Weibull 1995.) If, on the other hand, the larger the proportion the more F increases (i.e., $\partial^2 F/\partial p_s^2 > 0$), then the change would be typical of snowball processes: The larger the group of j_s's the stronger its impact on other strategies. Between these two extremes are processes for which the change is constant for all values of p_s:

$$\frac{\partial^2 F}{\partial p_s^2} = 0. \qquad (7)$$

In this case, the propensity to change is unaffected by either snobbish or populist considerations. This, as our next result shows, is the second important characteristic of the PFR.

THEOREM 2: *A propensity function* F *that is increasing in both variables satisfies conditions 6 and 7 if and only if the corresponding evolutionary dynamic is the PFR.*

Hence, we have linked properties of the PFR to clear and significant behavioral interpretations. This is critical to sustain a hope that Axelrod's conjectures apply to more than sticklebacks (Milinski 1987) and vampire bats (Wilkinson 1984). This section puts forward two conclusions that seem to be beyond doubt: The PFR is a plausible model of *some* behavioral processes, and it is *not* a plausible model of *all* behaviorally important processes. What happens under the PFR is the subject of this paper; what happens under other processes is an important subject of future research.

In this section we have shown that strong stability is impossible in iterated games. So is stability under all processes. Yet, weak evolutionary stability under the PFR may be feasible. This is the type of stability studied below. To avoid awkwardness, a strategy that is weakly evolutionarily stable under the PFR will be called "weakly evolutionarily stable" (weak ESS) or just "evolutionarily stable" (ESS) or "stable." We shall call the behavior of a population of ESSs an evolutionarily stable *state*. As we have shown elsewhere (Bendor and Swistak 1992, 1996c), a strategy i being weakly stable under the PFR is equivalent to i satisfying the following payoff condition: For all j,

$$V(i, i) \geq V(j, i), \text{ and if } V(i, i) = V(j, i), \text{ then } V(i, j) \geq V(j, j) \qquad (8)$$

Condition 8 is useful for a practical and a theoretical reason. First, it provides a simple test for establishing stability under the PFR. Second, it can be proven (Bendor and Swistak 1992, 1996c) that condition 8 is a borderline condition: A strategy is fundamentally unstable if and only if it fails to satisfy condition 8.

In the next section we establish some basic facts about weak ESSs in the IPD. These results will disprove several famous conjectures about TFT's "evolutionary advantage."

THE VARIETY OF STABLE STRATEGIES

To show that TFT has an evolutionary advantage over other strategies, it does not suffice to show that TFT has the desirable dynamic properties; it is equally essential to show that other strategies do not. As theorems 3–5 reveal, establishing that TFT has an advantage and figuring out why it does is more complex than a reading of Axelrod (1984) would suggest.

Evolutionarily Stable States Can Exhibit Any Amount of Cooperation

Since Axelrod's results emphasize two stable strategies,[14] ALL D and TFT, some researchers (Dawkins 1989) interpret his works as if only two evolutionarily stable states were possible: universal defection and universal cooperation. To clarify this issue we examine the feasible stable states and their underlying weak ESSs. We begin by asking how much cooperation can be achieved in a stable state.

We will say that a stable population state exhibits x amount of cooperation if x is the percentage of moves in which both players cooperate. This index is used by the next result.

THEOREM 3 (EVOLUTIONARY FOLK THEOREM FOR THE IPD): *An evolutionarily stable state in tournaments of iterated prisoner's dilemma with sufficiently high* δ *can exhibit any amount of cooperation.*

Hence, we have a profusion of stable states: from the least efficient state of pure defection to the most efficient state of pure cooperation. If we interpret the amount of cooperation in a stable state as the strategy's degree of "niceness," theorem 3 implies that ESSs can have any degree of niceness; thus *niceness and evolutionary stability are unrelated.*

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[14] Because weak evolutionary stability is a sound stability concept, unlike collective stability, and because most of Axelrod's results remain valid when redescribed in terms of the former, in the text we recast his results in terms of weak ESSs.

Evolutionarily Stable Strategies Can Be Infinitely Exploited

A more subtle problem is whether being *retaliatory*—TFT's second crucial property—is related to evolutionary stability. Being retaliatory confers an important benefit: Such strategies cannot be exploited in more than one period in a row. Thus, TFT's cumulative score against any j never falls below j's score by more than $S - T$. We call any strategy with this property *unexploitable*.[15] On the other end of the spectrum, we will call a strategy i *infinitely exploitable* if there is a strategy j such that $\lim_{\delta \to 1}[V(i, j) - V(j, i)] = -\infty$. Hence, an infinitely exploitable strategy loses to some other strategy j (in the sense of $V(i, j) - V(j, i)$) by an arbitrarily large amount as $\delta \to 1$. Intuitively, one might think that infinitely exploitable strategies cannot be evolutionarily stable. Yet our next theorem shows that they can.

THEOREM 4: *For any $0 < x \leq 1$, there is an evolutionarily stable strategy in the iterated prisoner's dilemma which is infinitely exploitable and supports* x *amount of cooperation in the stable state.*

Together, theorems 3 and 4 imply that neither niceness nor unexploitability are related to evolutionary stability per se. The set of ESSs contains strategies with vastly diverse properties: from purely cooperative to purely defective, and from unexploitable to infinitely exploitable. The mere fact that a strategy, say, TFT, is an ESS is hardly meaningful: That implies little about its other properties.

Strategies with Minimal Clustering Can Be Infinitely Exploited

Axelrod (1984) claims that one reason TFT is superior is that it can invade a stable population of ALL Ds given some population structure (i.e., if TFT is more likely to interact with its clones than with ALL D), whereas ALL D cannot invade a stable population of TFTs with or without population structure. He further asserts that among all strategies which can invade ALL D in clusters, TFT requires the least clustering. The problem, again, is how unique are these properties of TFT. Theorem 5 gives the answers.

THEOREM 5:

(i) *With sufficient clustering,* any *pure strategy that is not a neutral mutant of ALL D can invade a population of ALL D.*

(ii) *Among strategies that require the least clustering to grow in a population of ALL D, there are strategies which are both evolutionarily stable and infinitely exploitable.*

Thus, if invaders can cluster by freely setting the probability of interacting with their clones, then virtually any strategy—even the fundamentally unstable ALL C—can invade ALL D. This property does not make TFT distinctive.

Part (ii) of theorem 5 addresses a more demanding question: How diverse are the strategies which, like TFT, not only require the least clustering to increase among ALL D but also stabilize once common in the ecology? The answer: They are diverse indeed, ranging from the unexploitable TFT to strategies that are infinitely exploitable.

Theorems 3–5 show that arguments made about TFT's "evolutionary edge" can be made about a great many strategies, some of which differ profoundly from TFT. The evolutionary advantage of TFT and other conditionally cooperative strategies remains an open question. This question is answered in our next section.

WHY COOPERATION IS THE ROBUST STABLE STATE AND WHICH STRATEGIES SUPPORT IT IN "GAMES OF COOPERATION"

The Domain: Games of Cooperation

When thinking about the results on the evolutionary advantages of conditional cooperation, one must wonder whether they generalize beyond the prisoner's dilemma. Though the PD paints the problem of cooperation in its sharpest form, many other games exhibit, in varying degrees, problems of cooperation or coordination. Snidal (1991), for example, analyzes six such 2×2 games. Our main results, below, do indeed hold for a class of games much larger than the PD. Our first task, then, is to define these games, which we call "games of cooperation." In a set of symmetric two-person games with M actions, $a_1, \ldots, a_M$ $(M > 1)$, a game of cooperation is defined by the following two conditions that generalize two important properties of the PD: the value of cooperation and the danger of exploitation. (Below, $v(a, b)$ denotes player 1's payoff in a stage-game when she plays action a against player 2's b.)

The Value of Cooperation. In the PD, mutual cooperation is the unique socially efficient outcome: in addition to $R > P$, it is also assumed that $2R > T + S$. Similarly, we assume that in any game of cooperation there is a cooperative action, a_c, such that $v(a_c, a_c)$ is the unique efficient outcome in pure strategies, that is, $2v(a_c, a_c) \geq v(a_k, a_m) + v(a_m, a_k)$, for all m, k, equality holding if and only if $m = k = c$.

The Danger of Exploitation. The other key feature of the PD is the risk that a cooperating player may be exploited if his partner defects. In general, it makes sense to consider an action exploitive only if exploiting is at least as good as being exploited. Hence, our second assumption is that there exists an action, a_d, distinct from a_c, such that $v(a_d, a_j) \geq v(a_j, a_d)$ for all $j = 1, \ldots, M$. Together, the two assumptions imply

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[15] Formally we call a strategy i unexploitable if for all strategies j, all periods n in the game, and all values of δ, $V^n(i, j) - V^n(j, i) \geq S - T$, where V^n is the payoff in the game's initial n periods. Unexploitability in this sense means unexploitable in more than one period in a row. Because "pure unexploitability" $(V^n(i, j) - V^n(j, i) \geq 0)$ is an attribute only of nasty (never the first to cooperate) and retaliatory strategies such as ALL D, it makes cooperating impossible.

FIGURE 3. Two Games of Cooperation

	low	middle	high
low	2, 2	-2, 4	0, 2
middle	4, -2	0, 0	-4, 1
high	2, 0	1, -4	-2, -2

Game 1: military investment

	help	ignore	hurt
help	4, 4	-2, 7	-6, 2
ignore	7, -2	0, 0	-4, -2
hurt	2, -6	-2, -4	-3, -3

Game 2: bureaucratic politics

that the payoff to an exploited cooperator, $v(a_c, a_d)$, is less than the cooperative payoff, $v(a_c, a_c)$.

Any stage-game that satisfies these two conditions of cooperation and exploitation we call a *cooperation game.*[16] To see how these conditions generalize the PD, observe that in a game with two actions they imply only that $R > P$ and $2R > T + S$ (efficiency) and $T \geq S$ (exploitation). Hence, 2×2 games of cooperation include both kinds of stag hunt (assurance) and the kind of chicken in which compromise is Pareto-superior to alternately aggressing and conceding. (Indeed, five of the six games in Snidal's classification are included.)

Furthermore, because games of cooperation can have any finite number of actions, going beyond the 2×2 case generates an explosive increase in the number and variety of substantively important strategic situations. It also produces games with qualitatively new properties. Consider the two games in Figure 3.

Game 1 depicts a game of the military investment needed to seize valued territory. Because investment is costly, each side wants to avoid overspending. As a result, the game has no pure strategy equilibria. Yet, game 1 is a game of cooperation. In comparison, all of Snidal's games—indeed, *all* symmetric 2×2 games—have pure strategy equilibria. Thus, the conjunction between cooperation problems and the existence of pure strategy equilibria turns out to be a contingent feature of 2×2 games.

Game 2 represents a generic situation of bureaucratic politics in which each official can help, ignore, or hurt the other. This game of cooperation captures an important distinction between action and inaction (March and Simon 1958, 175). The prototypical "defection" in many organizational situations is inaction—the only costless, hence very common, solution. Punishment is another matter, for it absorbs resources. Because *no 2×2 game can distinguish between helping, ignoring, and hurting,* games with only two actions cannot possibly serve as plausible models of these and many other similar situations.

Because a game of cooperation is a generalization of the one-shot PD, when we turn to iterated games of cooperation, properties of strategies are straightforward generalizations of definitions adopted for the IPD. Thus, a strategy is nice if it never deviates from cooperation (a_c) first. A strategy is retaliatory if it punishes (plays a_d) any deviation a_k ($k \neq c$) from a_c until j's benefits from deviating are made up for by j's subsequent cooperation:

$$v(a_k, a_c) + Nv(a_c, a_d) \leq v(a_c, a_k) + Nv(a_d, a_c) \qquad \textbf{(9)}$$

where N is the minimal positive integer for which condition 9 holds for all k. We call a strategy nasty if it never cooperates first. Similarly, definitions of strategies in the IPD, such as TFT, extend directly to games of cooperation.

The Main Results

The property that turns out to distinguish cooperative ESSs like TFT harks back to the metaphor of balls rolling on hills and valleys. Recall that a ball in a deeper valley is more stable since it can absorb bigger shocks. Analogously, native strategies that can withstand large invasions are more stable. Thus, now we focus attention on this critical parameter of an evolutionary game—the minimal frequency that ensures a strategy's stability in the population. (See the Appendix for its precise definition.)

Compare two strategies, i and j, where i requires a frequency of at least 0.6 to be stable, whereas j requires a frequency of at least 0.9. Strategy i has a clear advantage over j. First, it is more likely than j to reach a stable state: If each starts out as a rare mutant, it is easier for i—other factors equal—to reach its critical threshold of 60% than for j to reach its threshold of 90%. Second, having become common in the population, i is more stable than j since it can resist invasions by larger groups: 0.4 of the population as opposed to 0.1 for j.

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[16] A cooperation game has a single "cooperative" action, a_c, but it may have more than one "defection" action.

Stabilizing frequencies are therefore of fundamental importance for both the emergence and the stability of strategies and their corresponding stable states. When two strategies have different minimal stabilizing frequencies, the one with the lower frequency is clearly "more stable." The key problem considered in this section is what strategies are the most stable (i.e., have the lowest stabilizing frequency) and what stable states (behavior) they support.

It is important to note that theorems 6–9 are formulated in a nontechnical way. This may be appreciated by some; others may find it insufficiently precise. The Appendix provides definitions, notation, and the "proper" formulation of theorems; the proofs remove any remaining ambiguities.

"Minority" Strategies Will Die Out in Some Ecologies. We begin our analysis by establishing the lower bound of the minimal stabilizing frequency in evolutionary games of cooperation with sufficiently high δ.

THEOREM 6: *In evolutionary games of cooperation with sufficiently high δ no strategy has a minimal stabilizing frequency smaller than 0.5 under any evolutionary dynamic.*

More precisely, consider any strategy i and an ecology in which it has a frequency of $p < 0.5$. Consider now a set of all evolutionary games with a p fraction of i and a fixed δ. Theorem 6 says that for sufficiently high δ this set includes ecologies in which i will decrease across generations. Hence, no strategy that is in the minority can be stable in all evolutionary games with sufficiently high δ—stability requires majority status of even the most robust strategy. The best we can expect is that the stabilizing frequencies of the most robust strategies come arbitrarily close to one-half.

Nice and Retaliatory Strategies Are the Most Robust. As Axelrod suspected, TFT turns out to be superior to many ESSs, not only in the IPD but also in the entire class of games of cooperation. Our next theorem establishes that all nice and retaliatory strategies (TFT, grim trigger, etc.) are indeed the most robust: All they need to stabilize is to exceed the threshold of one-half. (Note that in contrast to theorem 6, which holds for all evolutionary processes, theorem 7 holds only for the PFR.)

THEOREM 7 (CHARACTERIZATION THEOREM, THE SUFFICIENT CONDITION): *Under the PFR, the minimal stabilizing frequency of any nice and retaliatory strategy in a game of cooperation converges to 0.5 as δ converges to 1.*

Theorem 7's dynamic implications are straightforward. Suppose a majority of the people in an ecology initially play, for example, TFT. The rest can play *any* mix of strategies. Theorem 7 then guarantees that the native's relative frequency will not fall over successive generations. Indeed, in Boyd and Lorberbaum's (1987) example, where the native is TFT and the mutants are TF2T and STFT, TFT, though less fit than TF2T, does better than average. Accordingly, if the evolutionary dynamic is the PFR, TFT *will increase*. Of course, because TF2T scores highest in this ecology, it will increase faster than TFT. Only STFT will decrease, its loss offsetting the others' growth. Once the population restabilizes, only TFT and TF2T will remain; STFT will have died out. Because only nice strategies persist in the new equilibrium, the behavior of the old equilibrium—universal cooperation—is restored.

This point holds more generally. In fact, the proof of theorem 7 implies that, when in the majority, a nice and retaliatory native will score above average if the ecology has any nonnice strategies. With nonnice present, a nice and retaliatory native must increase. This process stops only when all nonnice mutants are wiped out. (See the proof of the theorem for one technical proviso.) Hence, the following corollary of theorem 7:

COROLLARY: *Under the conditions of theorem 7, the population converges to a stable state in which all strategies are nice. Hence, the resulting unique stable population state is universal cooperation.*

Thus, so long as mutants do not overwhelm a nice and retaliatory native i, their invasion will have no behavioral effect: If the future matters enough, the status quo ante of perpetual cooperation among all players will be reestablished. Of course, this does not mean that the ecology's composition would remain unaffected by the invasion: A nice strategy other than i may survive in the new equilibrium. In the example of {TFT, TF2T, STFT} the dynamics will stabilize with both TFT and TF2T surviving. As the corollary implies, the nonnice STFT will be eliminated.

In other situations, the nice and retaliatory native i may wipe out all mutants. Suppose, for example, that initially there are three strategies: i (which is in the majority), plus ALL C and ALL D. Depending on the game's parameters, the following trajectory is possible. ALL D may initially be the most fit and increase more quickly than i. (Theorem 7 guarantees i's increase.) But their increase is offset by the decrease of ALL D's prey, ALL C, which had given ALL D an evolutionary edge over i. The disappearance of ALL C will subsequently kill ALL D, which—absent ALL C—is less fit than i. In equilibrium only i will survive: The population will return to its original state.

Finally, it is most important to note one other property of the cooperative equilibrium induced by nice and retaliatory i. It may be difficult for a single pure strategy, say, TFT, to reach the stabilizing frequency of 50%. For instance, in Axelrod's simulations (1984, 51, Figure 2) no strategy grew beyond 15%. But note that theorem 7 *does not* require that i be pure; i may be a mixture of two or more nice and retaliatory pure strategies, say any mix of TFT and GT. Formally, i can be any mixed strategy, $i = p_1 j_1 + \ldots + p_k j_k$, of pure nice and retaliatory j's. And as long as the frequency of this mix is above 50%, it will not decline. (The composition of the mix may change, with some j's increasing and others decreasing, but its overall proportion in the ecology will be stable.) Thus, we can use theorem 7 to make inferences not only about a single strategy but also about sets of strategies. For example, in his first computer tournament Axelrod had three strategies that were nice and retaliatory: TFT, FRIEDMAN (GT), and SHUBIK. (Several other strategies were almost-nice and almost-retaliatory, as defined later.) Theorem 7 tells us that if any ensemble of these three strategies ever were to reach 50%, it would never decline. We can thus take an inductive observation, like that of Axelrod's Figure 2 (1984), and predict the ecology's evolution under certain hypothetical scenarios. Specifically, Theorem 7 implies that had any mix of TFT, FRIEDMAN, and SHUBIK been more than half of Figure 2's ecology, then, given sufficiently large δ, this group never would have decreased over simulated generations, and the stable state would have contained no defecting strategies. Theorem 7 and its corollary point to the importance of set-valued solutions in evolutionary games: What may be stable in some ecologies are sets of strategies rather than any single strategy in the set.

So far we have shown that all nice and retaliatory strategies enjoy the lowest possible stabilizing frequency in all games of cooperation with sufficiently high δ. We should interpret this finding cautiously, however, for it may be another "folk theorem" result: The same property may hold for a diverse set of strategies; worse, it may hold for all of them. Theorem 7 shows sufficient conditions for a strategy to be most robust. To make our findings meaningful, we must establish the necessary conditions as well. Another characterization result is thus needed.

The Most Robust Strategies Must Be Almost-nice and Almost-retaliatory. Consider a strategy (used in the proof of theorem 6) called MACHO, which begins by defecting. If its partner cooperates in period one, then MACHO defects always, but if its partner shows "toughness" by defecting in period one, MACHO cooperates in period two and thereafter (unless its partner ever defects again, whence MACHO retaliates forever). While vastly different, MACHO and TFT have one important property in common. When TFT plays another TFT it cooperates in all periods of the game; MACHO cooperates with its clone in every period but the first. Intuitively, the one-period defection should not much impair MACHO's robustness for large enough δ. This would also be true of all strategies which, when playing their clones, converge on uninterrupted cooperation. For this reason it makes sense to extend the notion of "niceness." We say that a strategy is almost-nice if it cooperates with its clone "almost always," in the sense that the payoff of an almost-nice strategy playing its clone converges (as $\delta \to 1$) to the efficient payoff garnered by two nice strategies: $\lim_{\delta\to 1} V(i, i)(1 - \delta) = v(a_c, a_c) = R$. Note that the set of nice strategies is a subset of almost-nice ones; hence, the latter concept generalizes the former.

Immediate provocability is yet another property of TFT that can be easily generalized. Consider a class of nice strategies called Tit for n Unreciprocated Exploitations (TFnUE). Any TFnUE cooperates in period one, thereafter defecting against any strategy j in any period $k + 1$ if the number of periods in which TFnUE was exploited (it cooperated, but its partner defected) exceeds the number of times in which TFnUE exploited its partner in the previous k periods by n. Operationally, then, TFnUE defects whenever its partner has defected n more times than it has. A common property of strategies like TFT and TFnUE is "some" degree of retaliation and unexploitability. This property of TFnUE ensures that $V(j, \text{TF}n\text{UE}) - V(\text{TF}n\text{UE}, j) \leq n(T - S)$; because no strategy j can outscore TFnUE by more than $n(T - S)$ for any value of δ, TFnUE is "almost-retaliatory." For payoffs this implies that, for all strategies j, $V(\text{TF}n\text{UE}, j)$ converges, as δ goes to 1, to at least the value of $V(j, \text{TF}n\text{UE})$. Formally we define strategy i as almost-retaliatory if, for all strategies j, $\lim_{\delta\to 1} V(i, j)(1 - \delta) \geq \lim_{\delta\to 1} V(j, i)(1 - \delta)$.

With these generalizations of TFT's two fundamental properties we can now formulate the conditions needed for maximal robustness.

THEOREM 8 (CHARACTERIZATION THEOREM, THE NECESSARY CONDITION): *For any evolutionary game of cooperation and any evolutionary dynamic, if the minimal stabilizing frequency of strategy* i *converges to 0.5 as* δ *converges to 1, then (i)* i *is almost-nice, and (ii)* i *is almost-retaliatory.*

It follows that neither strategies which fail to cooperate "almost always" with their clones (i.e., strategies which are not almost-nice) nor those which can be infinitely exploited (i.e., strategies which are not almost-retaliatory) attain the minimal stabilizing frequency. Maximally robust strategies, by virtue of being almost-nice, secure "almost universal" cooperation in the stable state. As δ approaches 1, their payoff comes arbitrarily close to the efficient payoff of universal cooperation.

Our last two results, theorem 9 and its corollary, tell us finally why defecting strategies are deficient. The corollary shows that the Hobbesian state of universal defection, supported by nasty strategies, is the most vulnerable of all stable states. It follows from a more general result, Theorem 9, which links a strategy's minimal stabilizing frequency to its degree of cooperativeness, or efficiency.

More specifically, for any game of cooperation denote $P = \min_{1\leq i\leq n} v(a_i, a_i)$ and $R = v(a_c, a_c) = \max_{1\leq i\leq n} v(a_i, a_i)$. Hence, for any strategy i, $R \geq \lim_{\delta\to 1} V(i, i)(1 - \delta) \geq P$. Writing $\lim_{\delta\to 1} V(i, i)(1 - \delta) = xR + (1 - x)P$, where $0 \leq x \leq 1$, we can meaningfully treat x as i's degree of efficiency. For any value of x consider a maximally robust strategy (one with the smallest minimal stabilizing frequency) in the class of strategies with x degree of efficiency and in games with δ converging to 1.

THEOREM 9: *For any evolutionary game of cooperation with sufficiently big* δ *and any evolutionary dynamic, the larger the degree of efficiency of the most robust* *strategy of this degree, the larger the maximal invasion this strategy can repel.*

COROLLARY: *For any evolutionary game of cooperation and any evolutionary dynamic, the minimal stabilizing frequency of any nasty strategy converges to 1 as* δ *converges to 1.*

Thus, as $\delta \rightarrow 1$, nasty strategies can be invaded by an arbitrarily small proportion of conditional cooperators, with no clustering whatsoever. The suboptimal Hobbesian state of pure defection is the least robust of all evolutionarily stable states. More generally, theorem 9 shows that this holds in a precise quantitative sense for *all* degrees of efficiency: Ceteris paribus, more cooperative natives can resist bigger invasions than can less cooperative ones.

And so, the deductive conclusions of evolutionary analysis are sanguine after all. Axelrod's insightful inductive observations have proved to carry the seed of a universal principle. The fully cooperative equilibrium is indeed evolutionarily advantaged. The benefits of this efficient result come, however, at the cost of everlasting provocability; as in the dictum: "eternal vigilance is the price of liberty" (John Philpot Curran, speech on the right of election of the Lord Mayor of Dublin, July 10, 1790).

CONCLUSION

The unique power of a properly constructed theory—a formal theory, as some would say—is that it precisely defines all objects of its domain and their properties. Consequently, we can see exactly why certain properties imply others. We have shown here in what sense cooperative, retaliatory strategies are more stable than others: The difference is in their minimal stabilizing frequencies. These results strongly agree with our empirical intuitions. In part this is because the minimal stabilizing frequency is a property with obvious empirical relevance. We believe that studying basins of attraction of evolutionary equilibria is essential to any evolutionary analysis, and we hope our article will encourage this direction of research.

Related Results

Our analysis is limited by the assumptions of the standard evolutionary model. New models are generated by altering one or more of these assumptions. In general, these analyses show that a small change in assumptions can easily affect equilibrium selection.

Such is the case, for instance, when noise (on actions, strategies, or payoffs) is introduced into the analysis (Bendor 1993; Foster and Young 1990; Kandori, Mailath, and Rob 1993; Sugden 1986; Young and Foster 1991.) Even models of the same type of noise can yield sharply contrasting conclusions. For example, although Boyd (1989) and Fudenberg and Maskin (1990, 1993) both study trembles—noise affecting actions—their conclusions are profoundly different: Boyd shows that for δ sufficiently big an evolutionary folk theorem holds, whereas Fudenberg and Maskin (1993) show that only "nearly" efficient states are stable! What drives the difference is that Boyd fixes δ and then introduces invasions, while Fudenberg and Maskin do the opposite.

Another set of assumptions that may profoundly affect equilibrium selection concerns the evolutionary costs of complexity. Some have suggested that since more complex strategies are harder to remember and implement, evolutionary dynamics will favor simpler ones. Binmore and Samuelson (1992) capture this idea via a lexicographic notion: If in a given ecology $V(i) = V(j)$ and i is simpler than j, then i is more fit than j; if their payoffs differ, complexity is irrelevant. However, measuring complexity is both controversial—there are several plausible ways to do it (Banks and Sundaram 1989)—and sensitive, since conclusions about evolutionary stability depend on these alternative measures. Because these results are relatively easy to present and nicely illustrate what may be a widespread problem with equilibrium selection, we now take a closer look at these two works.

Binmore and Samuelson use Rubinstein's (1986) measure: Take the simplest automaton required to implement a strategy and count its states. So ALL D, which needs only a one-state machine, is simpler than TFT, which needs two. In contrast, Banks and Sundaram argue that a complexity measure should account for the amount of monitoring, measured by how often an automaton transitions from one state to another: One automaton is simpler than another if it is at least as simple on both dimensions (number of states and of transitions) and strictly simpler on at least one of them. Using Rubinstein's measure, Binmore and Samuelson show that with undiscounted payoffs only efficient outcomes are evolutionarily stable in a large class of games including the IPD. Using their more comprehensive measure, Banks and Sundaram show that *no* outcome is stable.[17] Why the difference?

These authors agree on one point: Anything less than 100% cooperation is unstable. What they disagree about is the stability of full cooperation. Since a cooperative native is protected from invasions by "more efficient" strategies, it can only be invaded by strategies that are as efficient but simpler.[18] Binmore and Samuelson construct a two-state strategy, TAT FOR TIT, that is both stable and efficient, cooperating with itself in all but the first period. But for Banks and Sundaram, this strategy can be invaded by its unconditional neutral mutant, "defect in period one and cooperate thereafter." Thus for them, efficiency-based invasions destabilize zero cooperation, and simplicity-based invasions wreck everything else. A small difference in measuring complexity can profoundly affect equilibrium selection.

Above we chose to comment on a handful of papers on the evolution of cooperation. There are hundreds of others.[19] Our choice was motivated by two reasons. First, we wanted to illustrate what we think is the most important methodological lesson to be learned from the research on evolutionary games: "The very fact that varying the details in a dynamic model can alter the equilibrium selected shows that the institutional environment in which a game is learned and played can matter" (Binmore and Samuelson 1994, 1). Second, we wanted to focus attention on models that are most directly comparable with ours. Literally hundreds of other papers on evolutionary equilibria build on assumptions that are significantly different from the standard ones. For instance, stochastic evolutionary analysis has focused almost exclusively on one-shot games (e.g., Ellison 1993; Foster and Young 1990; Fudenberg and Harris 1992; Kandori, Mailath, and Rob 1993; Young 1993a, b). Due to the profound differences in assumptions, comparing results of these models might confuse rather than illuminate our understanding of the evolution of cooperation.

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[17] This sharp difference persists when payoffs are discounted; see Bendor and Swistak (1996b).

[18] The following illustrate these two types of invasions. (1) With undiscounted payoffs, TFT can invade ALL D. This is an efficiency-based invasion. (2) If the native is a GT, it can be invaded by ALL C, which discards the unused punishment state. This is a simplicity-based invasion.

Extensions

It is important to investigate how sensitive equilibria in evolutionary games are to changes in the game's standard parameters.[20] We suspect, however, that the most fascinating problems will arise along some relatively unorthodox lines of research. Consider the following example. Like most of evolutionary game theory, the general instability result of Bendor and Swistak (1992, 1996c) and Boyd and Lorberbaum's (1987) instability result for the IPD rely on a tacit assumption about strategies: Player 1's moves against player 2 depend only on the history of moves between 1 and 2. Hence, in Boyd and Lorberbaum's example, TFT's behavior vis-à-vis TF2T is based only on TF2T's conduct toward TFT. It turns out that if TFT can defect against TF2T to penalize it for "unwarranted" cooperation with STFT, then TFT will score highest in this ecology. But this requires departing from dyadic behavior: Player 1 must base her actions toward 2 not only on 2's history with 1 but also on 2's history with other players. Such strategies are norm-like in that 1 can punish 2 for 2's conduct toward third parties. Elsewhere we have shown that *norm-like* strategies are stable under all evolutionary processes (Bendor and Swistak 1993), not only under the PFR. This suggests that certain dynamics more typical of *homo economicus*—such as replication by switching to the winning strategy—may create a selective pressure for the evolution of norms. (See also Boyd and Richerson 1992.) The effect of other forms of social institutions on the evolution of cooperation has also been studied. These include population structure (e.g., Myerson, Pollock and Swinkels 1991; Pollock 1988), exit (e.g., Peck 1993, Vanberg and Congleton 1992), ostracism (Hirshleifer and Rasmusen 1989), and reputation (e.g., Raub and Weesie 1990). These and related studies go beyond the standard set of game parameters analyzed by economists and point up the importance of social and political institutions for the efficiency of equilibria.

Whichever direction the field goes, the canonical case would always serve as a reference point. If we do not understand the standard model, results obtained in other models are, to a large extent, meaningless. Hence, the objective of this article was to take the problem defined by the assumptions used in Axelrod's pioneering work and solve it deductively. Our answers, we believe, bring closure to a set of intriguing conjectures posed by *The Evolution of Cooperation.*

APPENDIX

Notation and Definitions

Denote by g a symmetric (identical sets of actions and a symmetric payoff matrix) two-player game, with a finite set of actions A. For $a_k, a_m \in A$, $v(a_k, a_m)$ denotes the payoff to the player moving a_k against the other player's action a_m, where v is a real-valued function defined on all pairs of actions: $v : A \times A \rightarrow Re$. An infinitely repeated game $G(g, \delta, S)$ of a stage game g is defined in a standard way; the third argument, S, denotes a set of strategies in the repeated game. We assume here games with discounting: $V(i, j) = \Sigma_{t=0}^{\infty} \delta^t v(a_i^t, a_j^t)$ denotes a payoff to i when it plays j; a_i^t and a_j^t, a_i^t, $a_j^t \in A$, are realized actions of i and j in period t. Parameter δ can be interpreted as a discount parameter, a probability of continuing the game, or a joint effect of both. An *evolutionary game EG*, where $EG = \langle G(g, \delta, S), E^T, F \rangle$ consists of a repeated game $G(g, \delta, S)$, an ecology of strategies playing the repeated game $E^T = ((j_1, p_1), \ldots, (j_N, p_N))$ in generation T (p_k is a frequency of strategy j_k in the population), and an evolutionary process F. Whenever the time index T is inessential we skip it. F is defined as any dynamic which for all strategies j_h and j_k satisfies $\Delta p_h / p_h < \Delta p_k / p_k$ iff $V(j_h) < V(j_k)$ and $\Delta p_h / p_h = \Delta p_k / p_k$ iff $V(j_h) = V(j_k)$, where $(p'_1, \ldots, p'_N) = F((p_1, \ldots, p_N))$ and $\Delta p_i = p'_i - p_i$. V's denote strategies' fitness ($V(j_k) = p_1 V(j_k, j_1) + \cdots + p_N V(j_k, j_N)$) in generation T whereas p and p' denote proportions in two consecutive generations T and T', respectively. More specifically, for any ecology $\{(j_1, p_1), \ldots, (j_N, p_N)\}$ we define simplex S^N as $S^N = \{p \in \Re^N : \Sigma_{i=1}^N p_i = 1$, where $p_i \geq 0$, for all $1 \leq i \leq N\}$. We define an evolutionary process as any function $F : \Re^N \rightarrow \Re^N$ that satisfies the two conditions above and leaves S^N and its faces invariant. This means that F transforms a vector of frequencies of strategies $j_1, \ldots, j_N$ from one generation $(p_1, \ldots, p_N)$ to another $(p'_1, \ldots, p'_N) = F((p_1, \ldots, p_N))$ such that $\Sigma\, p_i = \Sigma\, p'_i = 1$. F is a function of time (T), and as such it can be either discrete or continuous. A point $P \in S^N$ is *strongly stable* if there is a neighborhood of P such that in any point of the neighborhood the gradient of the process is strictly positive in the direction of P. A point P is *weakly stable* if the gradient is nonnegative. The concepts of weak and strong stability are closely related to the concepts of stability and asymptotic stability. If a process is sufficiently "well behaved" in the neighborhood of P (Lipschitz continuity is a sufficient condition), then strong stability implies asymptotic stability. If, however, a process is not Lipschitz continuous then strong stability need not imply asymptotic stability. In contrast, weak stability does imply stability for any process. Consider now any evolutionary game $EG = \langle G(g, \delta, S), E, F\rangle$ and any strategy j_k with frequency p_k in E: $(j_k, p_k) \in E$. Denote by $F|_k$ the truncation of F to its k^{th} coordinate. The minimal value of p_{k_0} such that for all ecologies E where $p_k \geq p_{k_0}$, $F|_k$ is nondecreasing in time is defined as j_k's minimal stabilizing frequency.

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[19] In their annotated bibliography, Axelrod and D'Ambrosio (1994) list 209 references for 1988–94. An earlier bibliography for 1981–87 (Axelrod and Dion 1987) lists 236 entries; the major findings of this period were summarized in Axelrod and Dion 1988.

[20] For example, sustaining cooperation when social efficiency requires specialization (e.g., $2R < T + S$ in the IPD) is probably harder than when it requires coordinating on the same action, but specialization is vital for complex institutions (Sugden 1986, 1989).

In all theorems below we assume that the set of strategies S is finite. S is also restricted to strategies that are either pure or finite mixes of pure strategies.

Theorems

We now proceed with the precise formulations of the theorems and their proofs. We begin with a result which is a simple consequence of theorems originally established in Bendor and Swistak (1992, 1996c). Because this result is a very useful characterization of strategies that are weakly stable under the PFR, it is useful in many of the following proofs.

Theorem A1: *A strategy is weakly stable under the PFR (proportional fitness rule) if and only if it satisfies condition 8.*

We now proceed to the proofs of theorems 2–9.

Theorem 2: *A propensity function* F *that is increasing in both variables satisfies conditions 6 and 7 if and only if the corresponding form of the evolutionary dynamic is the PFR.*

Proof: If F is the PFR then it clearly satisfies conditions 6 and 7. What requires proof is the inference in the opposite direction. Consider an ecology $E = \{(p_1, j_1), \ldots, (p_N, j_N)\}$. $\pi_{j_t j_s}$ stands for the probability that strategy j_t switches to j_s in the next generation. If by p_t^* we denote the proportion of strategy j_t in the next generation, then $p_s^* = \Sigma_{t=1}^N p_t \pi_{j_t j_s}$. Note that since for any $1 \leq t \leq N$, $\pi_{j_t j_s} = F(p_s, V^*(j_s))$, $\pi_{j_1 j_s} = \cdots = \pi_{j_N j_s} = \pi_{j_s}$, i.e., any strategy in the ecology has equal propensity to switch j_s. This implies that $p_s^* = p_1\pi_{j_s} + \cdots + p_N\pi_{j_s} = \pi_{j_s}$.

We will first show that $F(x, y) = Cxy$, $C > 0$. Without loss of generality we can assume that the domain of $F(x, y)$ is such that $0 \leq x \leq 1$ and $y > 0$, and that for $x, y > 0$ $F(x, y) = CxyG(x, y)$, where C is a constant not equal to zero. From condition 7 we have $F(Kx, y) = CKxyG(Kx, y) = KCxyG(x, y)$, for all $k > 0$. This implies that for $K > 0$ $G(Kx, y) = G(x, y)$, hence G is constant in x, and $F(x, y) = CxyG(y)$. Now, given that $F(x + \Delta, y) = F(x, y + \Delta)$, we conclude that $F(x, y) = C^*xy$. Moreover, $C^* > 0$, since F was assumed to be increasing in x and y.

Hence, we have $\pi_{j_t j_s} = F(p_s, V^*(j_s)) = Kp_sV^*(j_s) = p_s^*$. Given that $\Sigma_{s=1}^N p_s^* = 1$ and given that $V^*(j_s) = [V(j_s)]/V^*$, we conclude that $V^* = \Sigma_{s=1}^N p_sV(j_s) = \bar{V}$ and $K = 1$ so that, finally, $p_s^* = p_s[V(j_s)]/\bar{V}$. *Q.E.D.*

Theorem 3 (Evolutionary Folk Theorem for the IPD): *For an evolutionary game of IPD $EG = \langle IPD, E, PFR\rangle$ an evolutionarily stable state in games with sufficiently high δ can exhibit any amount of cooperation.*

Proof: Take a standard trigger strategy i (as in any proof of a folk theorem) that supports r amount of cooperation. For sufficiently high δ and any j which is not a neutral mutant of i we have $V(i, i) > V(j, i)$ which, given theorem A1 above, proves that i is evolutionarily stable. *Q.E.D.*

Theorem 4: *For an evolutionary game of IPD $EG = \langle IPD, E, PFR\rangle$ and any $0 < \varepsilon \leq 1$, there is an evolutionarily stable strategy which is infinitely exploitable and supports ε amount of cooperation in the stable state.*

Proof: Consider the following i. When playing a neutral mutant i cooperates every n^{th} period only, starting with period 1. When i plays a nonneutral j, then there is a period s when j moves differently than i. In this case i defects for the next $k - 2$ periods and cooperates in period $s + k - 1$, thereafter it only cooperates in every k^{th} period regardless of j's moves in the periods following s. Note that strategy i is infinitely exploitable by, e.g., ALL D. Take now any j which is not neutral with i. The best j can do against i is to defect in all periods. Thus $V(\text{ALL D}, i) \geq V(j, i)$, for all strategies j nonneutral with i. We will now determine a value of k which for sufficiently high δ will give $V(i, i) > V(\text{ALL D}, i)$, and thus $V(i, i) > V(j, i)$, for all nonneutral j (which implies evolutionary stability of i).

First, note that $V(i, i) = R/(1 - \delta^n) + P(1/(1 - \delta) - 1/(1 - \delta^n))$ and $V(\text{ALL D}, i) = T/(1 - \delta^k) + P(1/(1 - \delta) - 1/(1 - \delta^k))$. Consequently, we get $V(i, i) > V(\text{ALL D}, i)$ if and only if $(T - P)/(R - P) < (1 - \delta^k)/(1 - \delta^n)$. Take now any natural number m such that $(T - P)/(R - P) < m$. To show that there is an δ_0 such that for all $\delta \in (\delta_0, 1]$, $(T - P)/(R - P) < (1 - \delta^k)/(1 - \delta^n)$, denote $\delta^n = t$ and take $k = n(m + 1)$. Then $(1 - \delta^k)/(1 - \delta^n) = (1 - t^{m+1})/(1 - t) = 1 + t + \cdots + t^m$. Since $\lim_{t\to 1}(1 + t + \cdots + t^m) = m + 1$ and $1 + t + \cdots + t^m$ is an increasing function of t in $t \in [0, 1]$, there is a t_0, $0 < t_0 < 1$ such that, for all $t \in (t_0, 1]$, $m < 1 + t + \cdots + t^m$. Thus, equivalently, there is an δ_0 (namely, $\delta_0 = {}^n\sqrt{t_0}$) such that, for all $\delta \in (\delta_0, 1]$, $(T - P)/(R - P) < (1 - \delta^k)/(1 - \delta^n)$ which completes the proof that i is evolutionarily stable for sufficiently high δ. Since n can take any natural values, i, when universal in the population, can generate an amount of cooperation which is arbitrarily close to zero, being infinitely exploitable as well.

Q.E.D.

Theorem 5:

(i) *With sufficient clustering, any pure strategy in the evolutionary IPD that is not a neutral mutant of ALL D can invade a population of ALL D.*

(ii) *Among strategies that require the least clustering to grow in a population of ALL D, there are strategies which are both evolutionarily stable and infinitely exploitable.*

Proof:

Part (i). Consider an ecology with x i's and $(1 - x)$ j's. Denote: p = probability that i interacts with another i (note: $p \geq x$), q = probability that j interacts with another j. Then $V(i) = pV(i, i) + (1 - p)V(i, j)$ and $V(j) = qV(j, j) + (1 - q)V(j, i)$. Probabilities p and q are not independent and it is easy to show that $q = 1 - (x/(1 - x))\ (1 - p)$. Thus $V(i) = pV(i, i) + (1 - p)V(i, j)$, and $V(j) = (1 - (x/(1 - x))\ (1 - p))V(j, j) + (x/(1 - x))\ (1 - p)V(j, i)$. And so $V(i) > V(j)$ is equivalent to $p[(1 - x)V(i, i) + x(V(i, j) + V(j, i) - V(j, j)) - V(i, j)] > (1 - x)V(j, j) + x(V(i, j) + V(j, i) - V(j, j)) - V(i, j)$. With $A = (1 - x)V(i, i)$, $B = (1 - x)V(j, j)$, and $C = x(V(i, j) + V(j, i) - V(j, j)) - V(i, j)$, this becomes $p(A + C) > B + C$. Now, if j = ALL D then $B = (1 - x)V(j, j) = (1 - x)P/(1 - \delta)$. If i is a nonneutral invader of ALL D then playing another i it cooperates in at least one period. In consequence $A = (1 - x)V(i, i) > (1 - x)P/(1 - \delta)$. This implies that there is some $p < 1$ such that $p(A + C) > B + C$ which means that the nonneutral i can invade the population of ALL Ds if it clusters at the level p or higher.

Part (ii). Consider a strategy called FIRST IMPRESSIONS (FI). FI is defined as follows: (i) never defect first; (ii) if the opponent defected in period 1, defect in period 2 and thereafter; (iii) if the opponent cooperated in period 1 but defects in any subsequent period t, in $t + 1$ switch into "cooperate in every k^{th}" period, where k is determined as in the proof of theorem 4 for $n = 1$. Since FI is nice, and defects from period 2 on when playing ALL D, in the ALL D environment it behaves like TFT. Thus, FI satisfies Axelrod's definition (1984, 213) of a *maximally discriminating* strategy and from his Proposition 6 (p. 213), we know that such strategies can invade ALL D with the least clustering. Moreover, using the construction of the proof of theorem 4, FI can be shown to be evolutionarily stable. Finally, it follows directly from FI's definition that FI is infinitely exploitable. *Q.E.D.*

THEOREM 6: *For any evolutionary game of cooperation* $EG = \langle G(g, \delta, S), E, F\rangle$ *and any strategy* $i \in S$ *there is a* $\delta_0 < 1$ *such that i's minimal stabilizing frequency in games with* $\delta > \delta_0$ *is not less than 0.5.*

Proof: Take any evolutionary game of cooperation $EG = \langle G(g, \delta, S), E, F\rangle$ and any strategy $i \in S$. Assume that i's frequency is $0 < p < 0.5$ and that i plays in period 1 either the cooperative action a_c or some other a_m $(m \neq c)$ with probability one. If it plays a_c, assume the rest, $1 - p$, of the population play MACHO. If i plays a_m $(m \neq c)$ in period 1, assume that everyone else plays the following generalized version of grim trigger (GT): Play a_c (cooperate) in period 1, and never deviate from a_c first; if the opponent plays any other action a_m $(m \neq c)$ in any period k, play a_d from period $k + 1$ on. The best i can do against MACHO or GT (put j as standing for MACHO or GT) is by playing a_k that maximizes against a_d (i.e., $v(a_k, a_d) = \max_{1 \leq i \leq n} v(a_i, a_d)$) from period 2 onwards. Denote $m = v(a_k, a_d)$ and $M = v(a_d, a_k)$. Now, since $v(a_c, a_c) = R$ is the unique efficient payoff, the best i can do against itself is to play a_c, i.e., cooperate forever. Thus, we have $\max V^{2\rightarrow}(i) = \delta/(1 - \delta)[pR + (1 - p)m]$, and $\min V^{2\rightarrow}(j) = \delta/(1 - \delta)[pM + (1 - p)R]$, where $\delta \in (0, 1)$. Thus, since $M \geq m$ and $2R > m + M$, we get $\min V^{2\rightarrow}(j) - \max V^{2\rightarrow}(i) = \delta/(1 - \delta)[p(M - R) + (1 - p)(R - m)] \geq \delta/(1 - \delta)[(1 - 2p)(R - m)] > 0$ which increases without limit as δ goes to one. Hence, for sufficiently large δ, $V(i) < V(j)$, for all $p < 0.5$. This means that j will keep increasing under any evolutionary process F eventually driving i to extinction.

Consider now the case of $0 < p < 0.5$ i's that with probability $q \in (0, 1)$ play a_c (cooperates) in period 1. If i cooperates in period 1, then i's best move from period 2 onwards, for sufficiently high δ, is to cooperate forever with GT and with itself. If i deviates in period 1, then i's best move from period 2 onwards, for sufficiently high δ, is to play the best response against a_d against GT and to cooperate with itself. Let m and M be as defined above. Then $\max V^{2\rightarrow}(i) = \delta/(1 - \delta)[qR + (1 - q)[pR + (1 - p)m]]$, whereas $\min V^{2\rightarrow}(\mathrm{GT}) = \delta/(1 - \delta)[qR + (1 - q)[pM + (1 - p)R]]$. Thus, again, since $M \geq m$ and $2R > m + M$, we get $\min V^{2\rightarrow}(j) - \max V^{2\rightarrow}(i) = \delta/(1 - \delta)(1 - q)[p(M - R) + (1 - p)(R - m)] \geq \delta/(1 - \delta)(1 - q)[(1 - 2p)(R - m)] > 0$, and the rest of the reasoning proceeds as in the case above. *Q.E.D.*

THEOREM 7 (CHARACTERIZATION THEOREM, THE SUFFICIENT CONDITION): *For any evolutionary game of cooperation* $EG = \langle G(g, \delta, S), E, PFR\rangle$, *any* $\varepsilon > 0$, *and any nice and retaliatory strategy i, there is a* $\delta_0 < 1$ *such that i's minimal stabilizing frequency in games with* $\delta > \delta_0$ *is smaller than or equal to* $0.5 + \varepsilon$.

To prove theorem 7, the following lemma is useful.

LEMMA: *For any game of cooperation* $G(g, \delta, S)$, *any nice and retaliatory* $i \in S$ *and any nonnice* $j \in S$, $\lim_{\delta\to 1} V(i, i) - V(j, i) > 0$ *and* $\lim_{\delta\to 1} [V(j, i) - V(i, j)]/[V(i, i) - V(j, i)] \leq 0$.

Proof: Take any game of cooperation $G(g, \delta, S)$ with any nice and retaliatory strategy $i \in S$ and any strategy $j \in S$. Such i plays either a_c or a_d and every deviation of j from a_c (call it a_k) is followed by at least N periods of i's punishment. Moreover, i may return to cooperation after j's deviation a_k from a_c only if j cooperated in the preceding N periods (where $v(a_k, a_c) + Nv(a_c, a_d) \leq v(a_c, a_k) + Nv(a_d, a_c)$). In general strategy i either turns into permanent defection from some period on or it does not. We consider these two cases separately.

Case 1: there is an n such that in all periods $n' > n$ strategy i defects. Then $V(j, i) - V(i, j) = V^{\rightarrow n}(j, i) - V^{\rightarrow n}(i, j) + \sum_{l=n+1}^{\infty} \delta^{l-1}[v(a_{k_l}, a_d) - v(a_d, a_{k_l})] < K$, where $V^{\rightarrow n}$ denotes the sum of payoffs in periods 1 through n only, $a_{k_l} \in A$, and K is a positive constant. Clearly $\lim_{\delta\to 1}[V(j, i) - V(i, j)]$ is bounded from above because $v(a_m, a_d) \leq v(a_d, a_m)$ for all $a_m \in A$. Considering, moreover, that $v(a_c, a_c)$ is the uniquely efficient payoff, i.e., $2v(a_c, a_c) > v(a_{k_l}, a_d) + v(a_d, a_{k_l})$, and given that $v(a_{k_l}, a_d) \leq v(a_d, a_{k_l})$, the payoff to j against i in all periods following n is smaller than the payoff to i against i, $\lim_{\delta\to 1} V(i, i) - V(j, i) = +\infty$. The last two observations together give $\lim_{\delta\to 1} [V(j, i) - V(i, j)]/V(i, i) - V(j, i)] \leq 0$.

Case 2: i cooperates in an infinite number of periods. Suppose i returns to cooperation in period $m + 1$ after the first deviation, a_k, of a nonnice j in period n. Then $V^{\rightarrow m}(j, i) - V^{\rightarrow m}(i, j) = \delta^{n-1}[v(a_k, a_c) - v(a_c, a_k)] + \sum_{l=n+1}^{m-N+1} \delta^{l-1}[v(a_{k_l}, a_d) - v(a_d, a_{k_l})] + \sum_{l=m-N+2}^{m} \delta^{l-1}[v(a_c, a_d) - v(a_d, a_c)] \leq \delta^{n-1}[v(a_k, a_c) - v(a_c, a_k)] + \delta^{m-1}N[v(a_c, a_d) - v(a_d, a_c)]$. Given (9), $\lim_{\delta\to 1}[V^{\rightarrow m}(j, i) - V^{\rightarrow m}(i, j)] \leq 0$. Similarly (formally the proof ought to proceed by induction), for any period $m + 1$ in which i returns to cooperation $\lim_{\delta\to 1}[V^{\rightarrow m}(j, i) - V^{\rightarrow m}(i, j)] \leq 0$. (The same holds if in period $m + 1$ cooperation is merely continued, i.e., not necessarily restored.) Now given that i cooperates in an infinite number of periods, $\lim_{\delta\to 1}[V(j, i) - V(i, j)] \leq 0$. Since $v(a_c, a_c)$ is uniquely efficient, for all values of δ, $V(i, j) + V(j, i) < 2V(i, i)$, and given that $\lim_{\delta\to 1}[V(j, i) - V(i, j)] \leq 0$, for sufficiently large δ, $V(i, i) > V(j, i)$. Thus, $\lim_{\delta\to 1}[V(i, i) - V(j, i)] > 0$ and finally $\lim_{\delta\to 1} [V(j, i) - V(i, j)]/[V(i, i) - V(j, i)] \leq 0$. *Q.E.D.*

Proof of Theorem 7: Consider any evolutionary game of cooperation $EG = \langle G(g, \delta, S), E, PFR\rangle$ with an ecology $E = ((i, p), (j_1, p_1), \ldots, (j_N, p_N))$, where $i \in S$ is a nice and retaliatory strategy, and j's $\in S$ are any mix of strategies. Consider any $\varepsilon > 0$ and assume that the frequency of i is $p = 0.5 + \varepsilon$. We will show that for sufficiently large δ, this frequency is sufficient to stabilize i in all ecologies evolving under the PFR. This, in turn, demonstrates that i's minimal stabilizing frequency in games with sufficiently large δ is smaller than or equal to $0.5 + \varepsilon$. (Since the summation in all expressions below goes from 1 to N, we will skip the sum limits to increase the clarity of notation.) First, note that if all j_n are nice then $V(i) = V$, and i is stable under the PFR. Hence, the only nontrivial case to consider is when at least one j_n is not nice. In this case we want to show that for sufficiently high δ, $V(i) > pV(i) + p_1V(j_1) + \cdots + p_NV(j_N)$, which is equivalent to $(1 - p)V(i) - \sum p_nV(j_n) > 0$, i.e., that i's fitness is larger than the population average and hence i's frequency will increase under the PFR. (One proviso is important here. The intuition that a gradient pointing toward an equilibrium will force the process to converge on this equilibrium point is only correct for the continuous time PFR. The PFR's discrete time version can generate jumps long enough to yield an orbit that spins out of any neighborhood of a point that is an equilibrium point of a continuous PFR. See Dekel and Scotchmer [1992] for an example, and Bendor and Swistak [1996c] for a more extensive discussion of this matter.) This inequality can be rewritten as $(1 - p)[pV(i, i) + \Sigma\, p_nV(i, j_n)] - [p\, \Sigma\, p_nV(j_n, i) + \Sigma\, \Sigma\, p_np_kV(j_n, j_k)] = p\, \Sigma\, p_n[V(i, i) - V(j_n, i)] + (1 - p)\, \Sigma\, p_nV(i, j_n) - \Sigma\, \Sigma\, p_np_kV(j_n, j_k) > 0$. Note, however, that $\Sigma\, \Sigma\, p_np_kV(j_n, j_k) = \Sigma\, \Sigma\, p_np_kV(j_k, j_n)$ and thus $\Sigma\, \Sigma\, p_np_kV(j_n, j_k)$ can be rewritten as $0.5\, \Sigma\, \Sigma\, p_np_k[V(j_n, j_k) + V(j_k, j_n)]$. But for any two strategies j_n and j_k, $V(j_n, j_k) + V(j_k, j_n) \leq 2v(a_c, a_c)(1/(1 - \delta)) = 2V(i, i)$, since in every period $v(a_t, a_s) + v(a_s, a_t) < 2v(a_c, a_c)$ for all t, s except for $t = s = c$. Hence, $0.5\, \Sigma\, \Sigma\, p_np_k[V(j_n, j_k) + V(j_k, j_n)] \leq \Sigma\, \Sigma\, p_np_kV(i, i) = (1 - p)^2V(i, i) = (1 - p)\, \Sigma\, p_nV(i, i)$. Therefore, $p\, \Sigma\, p_n[V(i, i) - V(j_n, i)] + (1 - p)\, \Sigma\, p_nV(i, j_n) - \Sigma\, \Sigma\, p_np_kV(j_n, j_k) \geq p\, \Sigma\, p_n[V(i, i) - V(j_n, i)] + (1 - p)\, \Sigma\, p_n[V(i, i) - V(j_n, i)] + (1 - p)\, \Sigma\, p_n[V(i, j_n) - V(i, i)] = \Sigma\, p_n[p[V(i, i) - V(j_n, i)] + (1 - p)[V(i, j_n) - V(i, i)]$ Thus, it is sufficient to show that $\Sigma\, p_n[p[V(i, i) - V(j_n, i)] + (1 - p)[V(i, j_n) - V(i, i)]] > 0$. If a j_n is nice then obviously $p[V(i, i) - V(j_n, i)] + (1 - p)[V(i, j_n) - V(i, i)] = 0$. Thus, it suffices to prove that for a nonnice j_n and sufficiently high δ

$$p[V(i,i) - V(j_n, i)] + (1 - p)[V(i, j_n) - V(i, i)] > 0 \qquad \textbf{(A-1)}$$

From the lemma, we know that $V(i, i) - V(j_n, i) > 0$ for sufficiently large δ. For such δ, a little algebra gives the following equivalent form of inequality A-1: $(2p - 1)/(1 - p) > [V(j_n, i) - V(i, j_n)]/[V(i, i) - V(j_n, i)]$ But $p = 0.5 + \varepsilon$, so $2p - 1 > 0$, and, from the lemma, $\lim_{\delta \to 1} [V(j_n, i) - V(i, j_n)]/[V(i, i) - V(j_n, i)] \leq 0$, thus for δ large enough A-1 holds. *Q.E.D.*

THEOREM 8 (CHARACTERIZATION THEOREM, THE NECESSARY CONDITION): *Consider any evolutionary game of cooperation* $EG = \langle G(g, \delta, S), E, F\rangle$. *If strategy i is such that for any* $\varepsilon > 0$ *there is a* $\delta_0 < 1$ *such that i's minimal stabilizing frequency in games with* $\delta > \delta_0$ *is smaller than* $0.5 + \varepsilon$ *then (i) i is almost nice, and (ii) i is almost retaliatory.*

Proof:
Part (i). Take any evolutionary game of cooperation $EG = \langle G(g, \delta, S), E, F\rangle$ and a strategy $i \in S$. i is almost nice if and only if $\lim_{\delta \to 1} V(i, i)(1 - \delta) = v(a_c, a_c) = R$. Given that the cooperative outcome is uniquely efficient, if i is not almost nice then $\lim_{\delta \to 1} V(i, i)(1 - \delta) = L$, where $L < R$. We will prove, by contradiction, that if i is not almost nice then i's minimal stabilizing frequency as δ goes to 1 must exceed 0.5 under any dynamics F. Denote $\max_k v(a_k, a_d) = m$. Let i be not almost nice. Taking j = GT we get the following: $\max \lim_{\delta \to 1} V(i, j)(1 - \delta) = m$, $\min \lim_{\delta \to 1} V(j, i)(1 - \delta) \geq m$ (since $v(a_d, a_k) \geq v(a_k, a_d)$), $\lim_{\delta \to 1} V(j, j)(1 - \delta) = R$, and $\lim_{\delta \to 1} V(i, i)(1 - \delta) = L$. Take now an ecology with p_0 i's and $(1 - p_0)$ j's, where p_0 is the minimal frequency which stabilizes i in games with all $\delta \geq \delta_0$ for some δ_0. Then $\lim_{\delta \to 1} V(i)(1 - \delta) \geq \lim_{\delta \to 1} V(j)(1 - \delta)$. Thus, since $\max \lim_{\delta \to 1} V(i)(1 - \delta) = p_0L + (1 - p_0)m$, and $\min \lim_{\delta \to 1} V(j)(1 - \delta) = p_0m + (1 - p_0)R$, we get $p_0 \geq 1/[1 + (L - m)/(R - m)]$, which gives $p_0 > 0.5$ since $R > m$. Moreover, since this construction involves only i and j, $V(i) \geq V(j)$ is the necessary condition for i's stability under all processes F. This means that $p_0 > 0.5$ holds for i's minimal stabilizing frequency under all F.
Part (ii). Note first that i is almost retaliatory if and only if $\lim_{\delta \to 1} V(i, j)(1 - \delta) \geq \lim_{\delta \to 1} V(j, i)(1 - \delta)$, for all j. Thus, if i is not almost retaliatory, then there is a j' such that $\lim_{\delta \to 1} V(i, j')(1 - \delta) < \lim_{\delta \to 1} V(j', i)(1 - \delta)$. Note that there is a set of infinitely many strategies j which follow the same path with all pure components of i as j'. From this set of j's, take a j which is almost nice. Consider now an ecology with p_0 i's and $(1 - p_0)$ such j's, where p_0 is the minimal stabilizing frequency of i as δ goes to 1. Denote $\lim_{\delta \to 1} V(j, i)(1 - \delta) = U$ and $\lim_{\delta \to 1} V(i, j)(1 - \delta) = Z$. (Recall that $U > Z$.) Since $\max \lim_{\delta \to 1} V(i)(1 - \delta) = p_0R + (1 - p_0)Z$, where $R = v(a_c, a_c)$ and $\min \lim_{\delta \to 1} V(j)(1 - \delta) = p_0U + (1 - p_0)R$ and given that p_0 is the stabilizing frequency, we get $p_0R + (1 - p_0)Z \geq p_0U + (1 - p_0)R$. Considering now that $2R - Z - U > 0$, this last inequality is equivalent to $p_0 \geq 1/[1 + (R - U)/(R - Z)]$, which for $U > Z$ implies that $p_0 > \frac{1}{2}$. Again, since this construction involves only i and j, $V(i) \geqq V(j)$ is the necessary condition for i's stability under all processes F. This, in turn, means that $p_0 > 0.5$ holds for i's minimal stabilizing frequency under all F. *Q.E.D.*

THEOREM 9: *Consider any evolutionary game of cooperation* $EG = \langle G(g, \delta, S), E, F\rangle$. *If strategy i's degree of efficiency is x, then for any* $\varepsilon > 0$ *there is a* $\delta_0 < 1$ *such that i's minimal stabilizing frequency in games with* $\delta > \delta_0$ *is larger than* $1/(1 + x) + \varepsilon$. *Moreover, the minimal stabilizing frequency of the most robust strategy in the set of strategies with x degree of efficiency converges to* $1/(1 + x)$ *as* δ *goes to 1.*

Proof: Take any evolutionary game of cooperation $EG = \langle G(g, \delta, S), E, F\rangle$ and any pure strategy $i \in S$ with frequency p in E. i either cooperates (plays a_c) or deviates (plays $a_k \neq a_c$) in period 1. Denote $P = \min_{1 \leq i \leq n} v(a_i, a_i)$ and recall that $R = v(a_c, a_c) = \max_{1 \leq i \leq n} v(a_i, a_i)$. Hence, for any strategy i, $R \geq \lim_{\delta \to 1} V(i, i)(1 - \delta) \geq P$. Writing now $\lim_{\delta \to 1} V(i, i)(1 - \delta) = xR + (1 - x)P$, where $0 \leq x \leq 1$, we can treat x as i's degree of efficiency. Take now construction and notation identical to those in the proof of theorem 6, i.e., if i cooperates in the first period take an ecology of $1 - p$ strategies MACHO, if i deviates take $1 - p$ grim triggers; j stands for either of the two strategies. Note that for $\max \lim_{\delta \to 1} V(i)(1 - \delta) = p[xR + (1 - x)P] + (1 - p)m$ and $\min \lim_{\delta \to 1} V(j)(1 - \delta) = pM + (1 - p)R$. For i not to go extinct in ecologies with sufficiently large δ we must have $p[xR + (1 - x)P] + (1 - p)m \geq pM + (1 - p)R$. But since $M \geq m \geq P$ and $R > m$, given $2R > M + m$ and $M \geq m$, we get $p[xR + (1 - x)m] + (1 - p)m \geq pm + (1 - p)R$ which, given $R - m > 0$, is equivalent to $p \geq 1/(1 + x)$. Hence, for any $\varepsilon > 0$ we can find a sufficiently large δ_0 such that in all games with $\delta > \delta_0$ the minimal stabilizing frequency of i exceeds $1/(1 + x) + \varepsilon$. This holds for all processes F since, as in the proofs of theorems 6 and 8, our construction involves two strategies only. The proof easily extends to the case of a mixed action of i in the first period: the construction is identical to that in the proof of theorem 6.

From the algebra above it easily follows that if a pure strategy i with x degree of efficiency is a "grim trigger" type of strategy (follow the clone's pattern of moves and retaliate against any deviation with permanent punishment) then i's minimal stabilizing frequency converges to $1/(1 + x)$ as δ goes to 1. *Q.E.D.*

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The Private Production of Public Goods: Organizational Maintenance, Managers' Objectives, and Collective Goals

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I reformulate Mancur Olson's by-product theory of collective action as a theory of resource allocation by interest group managers. I then test alternative hypotheses about managers' objectives drawn from exchange theory and commitment theory. Financial data for 16 environmental citizen groups show that the production of public goods is subsidized by other activities, and revenues from member dues are not affected by spending on public goods. Spending on selective incentives and information generates revenues but also may contribute to the pursuit of collective goals. Estimated marginal revenues from fund-raising and selective incentives show that environmental citizen group managers are not preoccupied with maximizing revenues. Rather, they seek to maximize either spending on public goods or net resources available for influencing public policy and the environment, subject to a budget constraint.

For more than thirty years, the common point of departure for positive theories of collective action has been Mancur Olson's ([1965] 1971) by-product theory. Despite Olson's assertion that interest group managers must use earnings from other activities to finance the production of public goods, very little of the subsequent literature has focused on managers' behavior. Olson himself only implicitly considers managerial objectives and how they relate to spending on different kinds of activities. Other scholars have differed over whether managers are preoccupied with group maintenance (Salisbury 1969) or are motivated by a personal commitment to the collective goals of the organization (Sabatier 1992, Sabatier and McLaughlin 1990).

I analyze the problem of sustained mobilization for collective action from the standpoint of group managers. I use the term "managers" to refer to employees or volunteers who have the authority to determine how group resources are allocated. I first estimate the effect that spending on different activities has on group revenues and then calculate marginal revenues given mean spending levels. These marginal revenues allow me to test alternative hypotheses about managers' objectives.

My results provide substantial support for Olson's central conjecture that the private production of public goods depends on earnings from other activities, but I also propose several refinements. First, activities produced by environmental citizen groups in my data set cannot easily be separated into those that promote collective goals and those that generate revenues. These groups frequently use selective incentives and directed communications to disseminate information about collective goals to members and potential members. Second, the effects of spending on different activities are not independent. Revenues generated by spending on selective incentives and information are contingent on the level of spending on public goods, such as research, advocacy, and conservation activities. Third, the connection between spending on public goods and revenues depends on the source of the revenues. Increased spending on public goods has no effect on member dues but does appear to cause increased revenues from contributions, gifts, and grants.

With respect to managers' objectives, my results are most consistent with the hypothesis that managers seek to maximize net resources available to influence public policy and the environment. My results are inconsistent with the hypothesis that managers maximize revenues, since they clearly spend less on fund-raising than this hypothesis implies. Managers also tend to spend less on selective incentives and information than the amount that would maximize total revenues but more than the amount that would maximize net earnings. This implies that there are other reasons to spend resources on the activities, such as a desire for large numbers of well-informed members.

I also find that empirical relationships between spending and revenues vary across groups and over time. The effects of spending on selective incentives and information and on fund-raising and recruiting depend on whether the group distinguishes between member dues and voluntary contributions. Empirical relationships between spending and revenues also shifted during the 1980s, when popular support for many of the collective goals advocated by environmental groups was growing. My finding that marginal revenues are inconsistent with budget maximization is nonetheless robust with respect to group type and time period.

The paper is organized as follows. I first reformulate Olson's by-product theory as a theory of resource allocation by interest group managers. I then show that the estimated marginal revenues from spending on different activities can be used to test alternative hypotheses about managers' objectives. The bulk of the paper consists of my empirical analysis of financial data for environmental citizen groups. Finally, I summarize

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the implications of my findings for positive theories of collective action.

A REINTERPRETATION AND EXPANSION OF THE BY-PRODUCT THEORY

Selective Incentives and Cross-Subsidies

Olson motivates his by-product theory by asking whether it is rational for self-interested individuals to contribute to the production of public goods from which they derive benefits. He argues that individuals who are unable to influence the quantity or quality of some public good by an appreciable amount will not contribute, absent coercion or some other form of inducement. Since private organizations (other than labor unions and some professional associations) lack the power to coerce contributions, they must obtain resources by supplying goods or services that are available only to persons who contribute. According to the by-product theory, group managers use earnings from these "selective incentives" to finance the pursuit of collective goals (Olson [1965] 1971).

Many authors have argued that Olson overstates the importance of material motivations, particularly when the costs of joining are not large. Yet, Olson himself argues that "large" members do not completely free ride and that some members of "noneconomic lobbies" having social, political, religious, or philanthropic objectives may be motivated by ideological rather than material concerns ([1965] 1971 35, 159–65). Other authors have emphasized that selective incentives can take many forms, including the purely psychic value of expressing one's views through a monetary contribution (Moe 1980, 1981; Salisbury 1969).[1] Sabatier (1992) argues, however, that this definition of selective incentives is so broad that it leads to nonfalsifiable hypotheses. He offers a distinct alternative to the by-product theory, labeled "commitment theory," which hypothesizes that the primary difference between members and free riders is the degree of commitment to the collective goals of the organization (see also Sabatier and McLaughlin 1990).

Many studies attempt to test whether selective incentives are necessary to induce individuals to participate in collective action (e.g., Cigler and Hansen 1983; Hansen 1985; Hildreth 1994; King and Walker 1992; Knoke 1990; Mitchell 1979; Moe 1980; Rothenberg 1989, 1992; Sabatier 1992), but none actually tests the proposition that the sustained production of public goods is dependent upon earnings from other activities. On the one hand, internal cross-subsidies may be necessary even if some people are motivated to contribute by support for a collective goal, so long as the resources they contribute do not keep pace with the amounts managers spend on public goods. On the other hand, cross-subsidies may not be necessary even if most individuals free ride, so long as spending on public goods induces sufficient contributions from other sources. Many groups obtain their revenues from a variety of different sources, and wealthy individuals, foundations, and other patrons often give far more than the minimum amount required to receive selective incentives (King and Walker 1992; Walker 1983, 1991).

Wealthy individuals and institutional patrons may be more likely to contribute to groups that spend large amounts on public goods for a number of reasons. First, spending on lobbying, litigation, and other forms of public goods may affect the amount and kind of information available to the public, leading to increased demand for the group's specific objectives (see Hansen 1985). Second, group size itself is a form of advertisement, since potential donors are more likely to know about and contribute to a large organization (Steinberg 1986, 511). Third, potential donors may view the percentage of total spending devoted to public goods as an estimate of the portion of their donation that will not be "wasted" on administration or other expenses (Posnett and Sandler 1989, Richer 1995).

In the analysis that follows, I test Olson's theory using financial data for environmental citizen groups. Citizen groups are interest groups whose members are primarily autonomous individuals, without regard for professional or occupational status (Walker 1991). Environmental citizen groups provide a particularly stringent test for the by-product theory because they resemble Olson's noneconomic lobbies and because both the selective incentives and the costs of joining tend to be modest (Johnson 1995, Mitchell 1979, Olson [1965] 1971). Thus, if the theory holds for these groups, it should also hold for a wide range of other private organizations producing public goods.

Managers' Objectives and Autonomy

The literature on the private objectives of interest group managers is quite thin. While managers' objectives are not part of the by-product theory, Olson at least implies that managers place some positive value on the production of public goods; otherwise, why spend resources on an activity that attracts no members and generates no revenues? More specific hypotheses about managers' objectives can be derived from exchange theory and commitment theory.

Exchange theory assumes that entrepreneurs who found interest groups are primarily interested in their own material well-being and security. As a result, they are preoccupied with group maintenance rather than the pursuit of collective goals (Salisbury 1969). Moe (1980, 1981), Sabatier and McLaughlin (1990), and Sabatier (1992) apply this logic to leaders of established organizations. Niskanen (1971) takes essentially the same view of managers in bureaucracies and nonprofit organizations generally,[2] and Tullock (1966) makes a similar argument with respect to charities. Niskanen (1971, 38) maintains that budget maximization is a good proxy for many other goals that self-interested managers might pursue, such as high salaries, staff perquisites, public reputation, and power. I therefore operationalize exchange theory with the hypothesis that managers allocate resources so as to maximize group revenues.

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[1] The usual typology of selective incentives includes material, solidary, and expressive benefits (Salisbury 1969). Solidary benefits, such as the enjoyment of social activities, require joint participation by many individuals. Expressive benefits are the psychic rewards received from expressing one's views, without regard to the consequences of that action. These have at times been confused with purposive benefits, which derive from the change in the quantity or quality of some public good that is caused by a contribution (Johnson 1995).

Commitment theory assumes that the degree of personal commitment to collective goals increases with the degree of involvement in the organization (Sabatier and McLaughlin 1990, Sabatier 1992). Group managers therefore should be motivated to a significant degree by a desire to pursue collective goals. A narrow interpretation of commitment theory might imply that managers seek to maximize sustained spending on public goods. More generally, managers who are committed to collective goals should want to maximize the net resources available to pursue those goals (see Steinberg 1986). For citizen groups, collective goals are to influence public policy or some external variable, such as environmental quality. Their ability to influence public policy depends on both the available financial resources and the number of members they can claim (Lowry 1993, Oliver and Marwell 1992, Walker 1991). Attracting and communicating with a large number of members is also a way to build an informed constituency for specific agenda items (Rothenberg 1988, 1992). Citizen group managers who are motivated by collective goals therefore may be willing to trade some amount of spending on public goods for spending on activities that attract and inform members.

Sabatier (1992) and Sabatier and McLaughlin (1990) compare managers' ideological views and policy preferences to those of group members but do not examine resource allocation. In a study of managers in a bureaucracy, Sabatier, Loomis, and McCarthy (1995) find some evidence that U.S. Forest Service officials seek to increase budgets, but they conclude that the most important factors affecting planning decisions are local coalitions and bureaucratic resistance to change. The author whose methodology is closest to mine is Steinberg (1986), who examines fund-raising by nonprofit service firms. He finds that such firms seek to maximize spending on services rather than maximize budgets, but he makes no distinction between spending on public or private goods and does not link his study to theories of collective action. Steinberg (1986, 514) also assumes that managers of the firms he studied do not care about the number of donors who contribute or the number of volunteers they recruit.

Group managers who are concerned about the financial solvency of the organization are not completely free to pursue their own objectives but may have substantial autonomy. Moe (1980, 1981) and Rothenberg (1991, 1992) note that individual members and supporters typically have imperfect information about a group's policy positions as well as the actions taken by group managers. Managers can strategically provide information through fund-raising, group publications, and other forms of communication. They also may provide different information to different internal constituencies in an attempt to manage the internal politics of the organization (Moe 1980, 1981). The ability of managers to manipulate members and supporters in this manner is limited, however, if there are comparable groups competing for the same resources (Hirschman 1970, Sabatier and McLaughlin 1992).

In the analysis that follows, I focus on the empirical relationships between spending on different activities and revenues and on alternative hypotheses about managers' preferences. My data do not contain any direct measures of manager or member preferences or the content of communications. I note, however, that all the data I use are publicly available and thus can be used by members, potential members, and financial patrons to learn about the decisions of group managers. Moreover, the environmental citizen groups that make up my sample compete with one another for revenues and members, so that the threat of exit by dissatisfied members and patrons is real (Bosso 1995, 1997; McCloskey 1992). Finally, the empirical results reported below show that revenues clearly do respond to resource allocations, so managerial autonomy is limited by the need to maintain financial solvency.

A Model of Resource Allocation

In this section, I develop a model of resource allocation by interest group managers. My approach follows James's (1983) model of multiproduct, nonprofit organizations. Johnson (1988) presents a similar model of interest groups in which he assumes that revenues are fixed.

Assume that group managers seek to maximize benefits from spending on a total of n activities and the number of group members they can claim, subject to the constraints that total revenues equal or exceed total costs, and all spending is nonnegative:

$$\underset{<Q_1, \ldots, Q_n>}{Max} B(M(Q_1, \ldots, Q_n), Q_1, \ldots, Q_n)$$

subject to:

$$R(M(Q_1, \ldots, Q_n), Q_1, \ldots, Q_n)) \geq C(Q_1, \ldots, Q_n);$$

$$Q_i \geq 0 \text{ for all } i;$$

where B is the manager's benefit function, Q_i is the quantity of resources allocated to activity i, M is the number of dues-paying members, R is total revenues, and C is the group's cost function. Assume further that the membership and revenue functions all exhibit zero or diminishing marginal returns to spending on all activities, revenues exhibit positive but diminishing marginal returns to membership,[3] and marginal benefits are constant or decreasing over a range that includes the observed data.

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[2] Niskanen (1971, 15) defines "bureaus" as "nonprofit organizations which are financed, at least in part, by a periodic appropriation or grant." This definition is broad enough to include the organizations in my data set, all of which receive contributions, gifts, and grants that are not given in exchange for specific goods and services. See Appendix C.

If benefits are maximized by spending positive amounts on all activities, then the following first-order conditions must hold for each group and year:

$$\left[\frac{\partial B}{\partial M}\frac{\partial M}{\partial Q_i}+\frac{\partial B}{\partial Q_i}\right]=-\mu\left[\frac{\partial R}{\partial M}\frac{\partial M}{\partial Q_i}+\frac{\partial R}{\partial Q_i}-\frac{\partial C}{\partial Q_i}\right],\quad \text{for all } i;$$

where μ is the "shadow price" of the budget constraint, or the change in maximum obtainable benefits due to an exogenous increase in revenues by one dollar. In addition, total revenues must be greater than or equal to total costs. If revenues are scarce so that the budget constraint is binding, then the shadow price is positive. If the budget constraint is not binding, then the shadow price is zero (Chiang 1984), and spending continues until marginal benefits from the last dollar spent are zero.

This model is simplified in several respects. First, membership should be a function of the price of membership and the kind and quality of selective incentives provided to each member. Aggregate spending on selective incentives is then determined by the number of members and the cost function. In Appendix A, I show that aggregate spending on selective incentives nonetheless has a diminishing marginal effect on membership, provided that any economies of scale and scope in producing selective incentives are not too large.[4] Second, it may be possible for spending to exceed revenues in a given year, as most groups have endowment funds that allow them to absorb a temporary shortfall. Alternatively, groups that cannot borrow from their endowment or some other source face the additional constraint that total spending must be less than the amount of funds on hand.

The connection among managerial objectives, revenue functions, and resource allocation can be seen by substituting specific objectives for the generalized benefits function. If managers seek to maximize total revenues, then we can substitute marginal revenues for marginal benefits on the left-hand side of the first-order condition and rearrange terms to get $[(dR/dM)*(dM/dQ_i) + dR/dQ_i] = (dC/dQ_i)*\mu/(1 + \mu)$. The sum inside the brackets represents total marginal revenues. If the units used to measure Q_i are dollars spent, then marginal cost is one by definition, and spending should continue until marginal revenues equal $\mu/(1 + \mu)$. If the budget constraint is binding, then the shadow price is positive, and marginal revenues at the efficient spending levels are greater than zero but less than one. If total revenues exceed total spending, then the shadow price is zero, and spending on each activity should continue until marginal revenues are zero. Note that marginal revenues will also be zero if spending on an activity simply fails to generate revenues, as Olson predicts is the case for public goods.

Alternatively, citizen group managers may seek to maximize net resources available to pursue collective goals. A large number of benefit functions could fall within this description, but consider the weighted sum of spending on each activity and the number of members: $B = \pi_1*Q_1 + \ldots + \pi_n*Q_n + \delta*M$, where π_i is the weight attached to spending on activity i, and δ is the weight attached to members.[5] Differentiating with respect to Q_i and substituting for marginal benefits in the first-order condition gives $[(dR/dM)*(dM/dQ_i) + dR/dQ_i] = 1 - (\pi_i + \delta*(dM/dQ_i))/\mu$. The exact value of the optimal spending level will depend on the weight attached to the activity itself (π_i), the weight attached to the number of members (δ), the marginal effect of spending on memberships (dM/dQ_i), and the shadow price (μ). Marginal revenues should be less than one for any activity that has a positive effect on benefits because it contributes directly to the pursuit of collective goals, or because it attracts members. Marginal revenues could exceed one for an activity that generates needed earnings but that managers actually dislike.

The hypothesis that managers maximize spending on public goods is actually a special case, where π_i is positive for public goods but zero for all other activities, and δ is also zero. Thus, marginal revenues should equal one for all activities other than public goods. If, contrary to Olson's free-riding assumption, spending on public goods does generate revenues, then marginal revenues from spending on public goods should be less than one but greater than zero. If Olson is correct, then marginal revenues from spending on public goods are zero by definition.

The general rule is as follows: Managers maximize benefits when they allocate resources to each activity so that the direct benefits from spending one more dollar and the indirect benefits from any change in membership exactly offset the net change in earnings. A finding that marginal revenues fall between zero and one for some activity is consistent with any hypothesis that asserts managers receive direct or indirect benefits

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[3] Marginal revenues may not be a continuously decreasing function of membership. Assume, for example, that a group conducts a direct mail fund-raising campaign. It initially targets individuals who are expected to contribute the most, resulting in diminishing marginal returns as the amount spent on direct mail increases (Rose-Ackerman 1987). If foundations use membership as a factor in deciding which groups to finance, foundation grants may increase once membership passes a threshold point. Sooner or later, however, additional spending on direct mail will fail to attract more foundation grants, and marginal revenues will again decline.

[4] The price of membership does not explicitly appear in my analysis. Minimum dues for many citizen groups are purposely kept low so that they will not have a significant effect on membership decisions (Johnson 1995, Rothenberg 1992). Minimum dues for groups in my data set are on the order of $10 to $30 and often remain constant for several years.

[5] Other variations could include concave functions of spending, interactions between spending on different goods, and separate weights for different kinds of supporters, such as "activists" and "checkbook" members. Managers also may maximize spending on certain activities subject to the additional constraint that membership remain above some level. All these variations are covered by the general rule that marginal revenues should be less than one for any activity that managers value for reasons other than the net earnings generated.

TABLE 1. Groups and Years Covered in the Data Set

Group	Years
American Forests (AF)	1992–94
American Rivers, Inc. (ARI)	1990–94
Center for Marine Conservation (CMC)	1989–94
Defenders of Wildlife (DOW)	1987–94
Environmental Defense Fund (EDF)	1986–93
Friends of the Earth (FOE)	1991–93
Izaak Walton League of America (IWL)	1988–92
National Audubon Society (NAS)	1984–93
National Parks and Conservation Association (NPCA)	1980–93
Natural Resources Defense Council (NRDC)	1989–93
National Wildlife Federation (NWF)	1990–93
Sierra Club (SC)	1976–93
Sierra Club Legal Defense Fund (SCLDF)	1990–94
Trout Unlimited (TU)	1991–93
Wilderness Society (WS)	1983–92
World Wildlife Fund (WWF)	1991–93

from spending on that activity beyond the earnings it can generate. Given that shadow prices and the weights attached to different terms in the benefits function cannot be observed, my ability to test specific hypotheses about managers' objectives depends on inferences that can be drawn due to the overall pattern of marginal revenues from spending on different activities.

APPLICATION TO ENVIRONMENTAL CITIZEN GROUPS

Description of the Data Set

Table 1 lists the groups and years included in my data set. I initially requested information and financial reports from 26 nationally prominent environmental citizen groups. These data were supplemented by annual reports published in magazines such as *Sierra* and *National Parks*, tax returns obtained from the Internal Revenue Service, and financial reports filed with state authorities.[6] My data set includes 16 groups for which I was able to distinguish spending on public goods from spending on selective incentives and information for three consecutive calendar years and which are not focused primarily on raising funds to purchase land.[7]

I standardized the data by first classifying all revenue and spending figures according to generic categories. Revenues were divided among member dues; contributions, gifts, and grants; net income from sales and events open to the general public; and other income.[8] Expenditures were divided among public goods; selective incentives and information; fund-raising and member recruiting; and administration. Fiscal year data were converted to calendar years according to the number of months in each calendar year, and all figures were converted to constant 1992 dollars using the CPI-U index. The first calendar year observation for each group was then dropped to allow for the use of lagged variables.

Several issues arose regarding the classification of revenue and expenditure data. On the revenue side, tax forms do not require that contributions, gifts, and grants be broken down by source,[9] and the detail provided in annual reports and financial statements varies. Many groups do list revenues from bequests separately in their annual report. These revenues can vary widely from year to year, and a single large bequest could easily distort my results. Therefore, I excluded bequests from my dependent variable where they are reported separately.

On the expenditure side, IRS Form 990 requires that all nonprofit organizations report separate totals for program activities, fund-raising, and administration but does not require that program spending be reported according to any particular categories. I divided program spending into either public goods or selective incentives and information on a case-by-case basis. I relied on descriptions provided in Part III of IRS Form 990, annual reports, audited financial statements, and statements filed with state governments.

Appendix B lists the categories appearing in source documents that are classified as public goods or selective incentives and information for purposes of my analysis. "Public goods" includes policy advocacy directed at government decision makers, litigation, scientific research, and conservation projects, such as wildlife management programs; "selective incentives and information" includes member benefits and communications as well as other activities designed to enhance public awareness of environmental issues and problems.

The groups in my data set vary widely by the size of budget, the extent to which they rely on different kinds of revenues, and the way they allocate resources among different activities. Data on revenues and spending,

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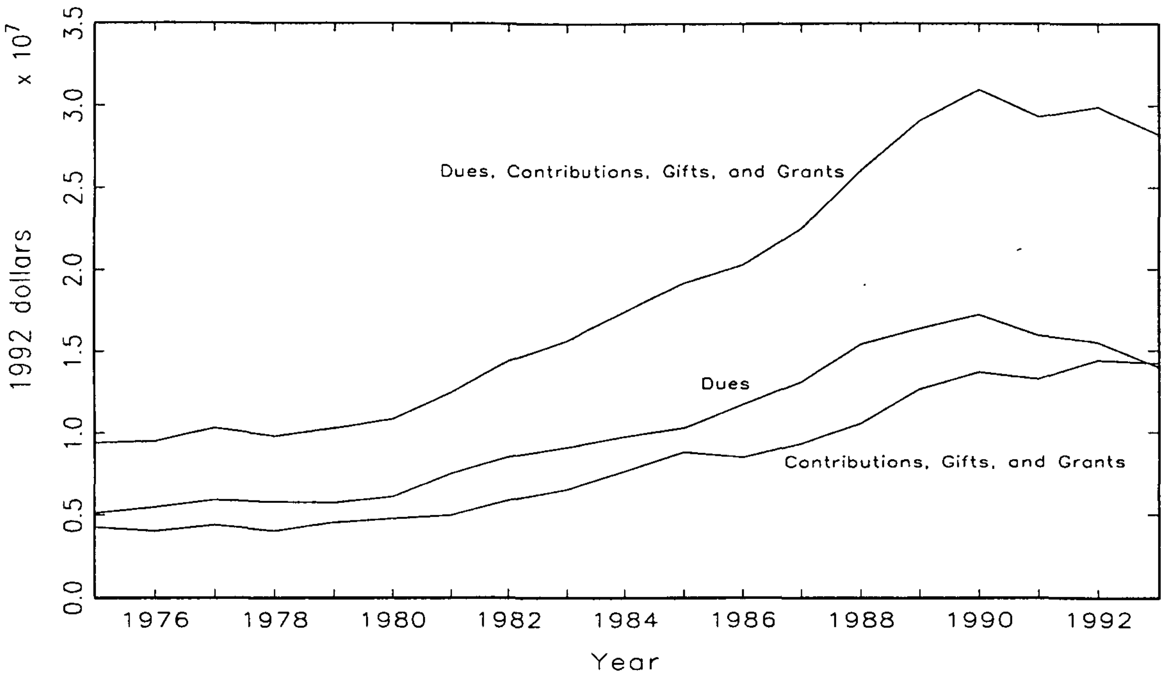
[6] Virtually all organized interest groups are legally structured as nonprofit corporations. Although they are exempt from federal corporate income taxes, they still must file annual returns on IRS Form 990. These returns are public information (except for the identities of specific contributors) and must be kept on file by the IRS for three years. Nonprofit organizations that solicit donations are also required to comply with some sort of registration and reporting requirements in 45 states (Hopkins 1993). The specific organizations that are covered and the information that must be provided differ by state, as do the number of years records are kept on file and the condition of the files.

[7] There is no conceptual reason that the by-product theory could not apply to groups that raise money to buy land, but preliminary tests based on 15 observations suggested that they have very different revenue functions. Groups omitted for this reason include the Nature Conservancy, Ducks Unlimited, and the Rocky Mountain Elk Foundation. Others that were omitted due to insufficient data are the Citizen's Clearinghouse for Hazardous Wastes, Clean Water Action, Environmental Action, the Earth Island Institute, Greenpeace, Public Citizen, and the Rainforest Action Network.

[8] "Sales and events" include earnings from merchandise sales, attorneys' fees (which represent payment for services), conventions, camps, outings, banquets, and other special events. "Other income" includes interest on endowment funds, sale of assets, mineral rights, royalties, and rental income from real estate or mailing lists.

[9] IRS Form 990, line 1, requires that groups list "contributions, gifts, grants and other amounts," broken down by "direct public support," "indirect public support," and "government contributions (grants)." Indirect public support includes contributions made through joint fund-raising activities, such as the United Way.

FIGURE 1. Sierra Club Revenues, 1975–93



and the percentages in each generic category for 1992, are shown in Appendix C. For that calendar year, revenues in the form of dues and public support averaged $15.78 million, net sales and events averaged $1.48 million, and other income averaged $1.85 million. An average of $7.28 million, or about 40% of spending, was on public goods, compared to $6.31 million on selective incentives and information, $3.44 million on fund-raising, and $1.38 million on administration. Differences between total revenues and spending are accounted for by changes in the value of the group's endowment fund.

Differences between Groups

Five groups in my data set—the Center for Marine Conservation, Environmental Defense Fund, Natural Resources Defense Council, Sierra Club Legal Defense Fund, and World Wildlife Fund—do not report member dues separately from contributions. They also tend to offer less in the way of selective incentives and opportunities for participation by volunteer activists than do the other groups in my sample (Lowry 1993).

It seems reasonable to suggest that empirical relationships between spending and revenues depend on internal group structure and the relative emphasis placed on collective or selective benefits (see King and Walker 1992, Moe 1981). While it may be possible to develop more precise measures of these characteristics, I simply take the fact that some groups do not have a separate category for dues paid in exchange for selective incentives as a proxy for other characteristics, and I test for empirical differences based on this distinction.

My data show that groups whose "members" probably do little more than write checks tend to devote a larger share of their resources to producing public goods than do groups whose members receive color magazines and are more likely to participate in state or local chapters. Appendix C shows that groups which do not distinguish between member dues and contributions devoted an average of 57.8% of expenditures to public goods and 21.2% to selective incentives and information during 1992; for groups that do make the distinction, the comparable averages are 37.1% and 35.7%.

Shifts in External Conditions

The revenues of environmental citizen groups may be affected by changes in external economic and political conditions (Hansen 1985, Johnson 1988, Richer 1995). I use real per-capita disposable personal income as a measure of external economic conditions. Revenues should increase with the disposable income of potential contributors. I experimented with the unemployment rate and the percentage change in personal disposable income as cyclical measures of prosperity, but the results did not reveal any significant effect.

The ability to raise revenues also may be affected by external shifts in political conditions and public opinion. The 1980s were a period of great prosperity for environmental citizen groups in terms of their growth in revenues and members (Bosso 1995, 1997, McCloskey 1992). This is illustrated by Figure 1, which shows real revenues (1992 dollars) for the Sierra Club from 1975 through 1993. While real revenues remained fairly flat during the late 1970s, they began increasing rapidly in 1981 and continued to rise until the end of the decade. Real revenues flattened out after 1990, and member dues actually declined from 1990 to 1993.

Bosso (1997) also shows that many environmental citizen groups experienced stagnant or decreasing membership and revenues in the early 1990s.

Many commentators attribute the growth of environmental citizen groups during the 1980s to opposition to Secretary of the Interior James Watt and EPA Administrator Anne Burford, plus a steady increase in popular support for environmental protection that ended shortly after the turn of the decade (Bosso 1997, McCloskey 1992, Vig 1997). One question included in the General Social Survey (GSS) asks whether respondents feel that too much, too little, or about the right amount is being spent on improving and protecting the environment. In 1980, 50.8% of respondents who answered the question said "too little." From 1982 through 1990, the percentage responding "too little" grew every year, reaching a high of 74.2% in 1990. In 1991, the figure dropped for the first time in a decade, to 71.1%. By 1994, only 62.3% of respondents believed too little was being spent.[10]

The results reported below test for whether the coefficients in my revenue functions shifted during 1981–90. This period begins with Ronald Reagan's inaugural and ends with passage of the 1990 Clean Air Act amendments and the 1991–92 recession. An affirmative finding implies that the revenues generated by environmental citizen groups depend on external conditions that affect the demand for collective goals as well as on group spending for specific activities.

I experimented with interaction terms for 1981–88 and 1981–92 as well as interaction between spending variables and the percentage of GSS respondents answering "too little." The results were similar in sign and magnitude, but the standard errors were much larger relative to the coefficients than are the results reported here. I also tried controlling for changes in external conditions by including a dummy variable for each year. Stimson (1985) recommends against using both intertemporal and cross-sectional dummies because of possible colinearity with the explanatory variables. In fact, the only explanatory variable that was much affected was disposable personal income, while the overall fit of my regressions did not improve. Finally, I tried including a single dummy variable for 1981–90 in addition to my interaction terms, but the absolute t-value for the intercept shift was less than 0.5 for each specification.

Specification of Revenue Functions

My revenue functions include a term for the interaction between spending on public goods and spending on selective incentives and information to determine whether the effect of one is contingent on the level of the other. King and Walker conclude that "potential members may be attracted to a group that meets two separate needs: a desire for selective material benefits *and* a commitment to collective goods" (1992, 422). This speculation echoes Olson's contention that the sufficient condition for collective action is "a joint offering or 'tied sale' of a collective and a noncollective good" ([1965] 1971, 133–4) and Moe's claim that "*both* political and nonpolitical considerations can be integral components of organizational incentive structures" (1981, 543).

I also include group spending on internal administration. Some potential donors may view high administrative expenses as a sign of inefficiency, which may tend to diminish contributions (Steinberg 1986). Alternatively, high spending on administration may indicate a more qualified, professional staff that increases the chances of attracting large grants from foundations and other patrons as well as provides support for members (see King and Walker 1992).

Of course, spending on different activities may itself be a function of revenues. Groups promise to provide certain benefits to members in exchange for dues, and many foundation grants specify that the funds be spent on public goods. If solicitations for additional contributions are routinely sent to current members, then fund-raising expenditures also may be a function of dues revenues. Finally, additional revenues may lead to an increase in various overhead functions, resulting in more administrative expenditures.

My empirical analysis assumes that (1) fund-raising and recruiting expenditures are not a function of *current* year revenues, and (2) revenues respond to lagged spending on other activities. Given these assumptions, it is not necessary to correct for endogenous independent variables. Steinberg (1986, 520) uses instrumental variables to estimate marginal revenues from fund-raising, but he concludes that the results "tell the same story" as the results from a model with lagged independent variables. Some of the interaction terms discussed below were not significant at conventional levels when I estimated a two-stage least-squares model, but otherwise the results were qualitatively the same as when I use lagged values.

I therefore use the following generic model for group j in year t, where $u_{j,t}$ is a stochastic term with mean zero:

$$\text{Revenues}_{j,t} = f(\text{Intercept}_j, \text{Disposable personal income}_t, \text{Spending on public goods}_{j,t-1}, \text{Spending on selective incentives and information}_{j,t-1}, \text{Spending on fund-raising and recruiting}_{j,t}, \text{Spending on administration}_{j,t-1}, u_{j,t}).$$

The results reported below were estimated using two different specifications. One alternative is a log-log specification, with both the dependent and independent variables measured in natural logs. This allows the spending coefficients to be interpreted as elasticities, or the percentage increase in revenues caused by an increase of 1% in spending (Chiang 1984). It also allows me to verify the assumption of positive but diminishing marginal revenues from spending, which holds for coefficients between zero and one.[11] The other specification uses the ratio of spending on public goods or selective incentives and information to total spending, which may be thought of as the inverse of the "price" to donors of a one-dollar increase in spending on each activity (Posnett and Sandler 1989, Richer 1995).[12] I use the inverse of this "price" so that the sign of the coefficient can be interpreted the same in both specifications.

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[10] General Social Survey data were originally collected by the National Opinion Research Center and were obtained from the Inter-University Consortium for Political and Social Research, "General Social Surveys, 1972–1994," Computer File No. 6492 (Davis and Smith 1974–94). The GSS was not conducted in 1981 or 1993. Figures are based on the combined results for variables NATENVIR, NATENVIY, and NATENVIZ. NATENVIY was used on GSS Form 2 in 1984–86, and forms 2 and 3 in 1987 asked whether too little, too much, or the right amount was being spent on "the environment." NATENVIR and NATENVIZ both asked whether too much, too little, or the right amount was being spent on "improving and protecting the environment" (Davis 1993, 114–21).

I also estimated a quadratic specification. The coefficients on the quadratic terms for disposable personal income, selective incentives and information, fundraising and recruiting, and administration are all significant at conventional levels, indicating that the correct specification is not linear. Marginal revenues from spending on different activities used to test hypotheses about managers' objectives are very similar to those derived from the log-log specification. My discussion of empirical results focuses on specifications that are more parsimonious and less subject to multicolinearity than is the quadratic model.

Hypotheses

I take as my first null hypothesis the strong version of Olson's free-rider assumption, as seen from the manager's perspective:

HYPOTHESIS 1: *Increases in spending by environmental citizen groups on public goods, including policy advocacy, litigation, research, and conservation projects, do not cause increases in revenues.*

I refer to this as the "strong" version because it assumes that no causal connection exists, as opposed to one that is small in magnitude or that exists only for certain revenue sources. Note that policy advocacy is distinct from messages contained in selective incentives or general public education about the environment. With respect to managerial objectives, I have three alternative hypotheses:

HYPOTHESIS 2A (EXCHANGE THEORY): *Environmental citizen group managers seek to maximize revenues. Marginal revenues from spending on all activities that generate revenues are significantly less than one but greater than or equal to zero.*

HYPOTHESIS 2B (NARROW COMMITMENT THEORY): *Environmental citizen group managers seek to maximize spending on public goods. Marginal revenues from spending on all other activities that generate revenues are equal to one. Marginal revenues from spending on public goods are less than one and may be zero.*

HYPOTHESIS 2C (BROAD COMMITMENT THEORY): *Environmental citizen group managers seek to maximize net resources available to influence public policy and the environment. Marginal revenues from spending on activities that increase both revenues and influence are less than one. Marginal revenues from spending on activities that are produced only to generate earnings are equal to one. Marginal revenues from spending on public goods are less than one and may be zero.*

These three hypotheses follow from the assumptions that managers maximize benefits subject to a budget constraint and that marginal revenues from spending on each activity are zero or decreasing. If marginal revenues from some activity equal marginal cost (one in this case), then the activity is only being pursued in order to generate earnings that can be used to finance some other activity. If marginal revenues are less than marginal cost, then managers place a positive value on the activity itself, such that they are willing to accept lower earnings in order to spend more on it. Finally, recall that marginal revenues from some activity could be zero either because there is no causation or because managers spend until no further revenues are generated by the last dollar spent.

In order to test hypotheses about managers' objectives, we need to look at the revenue function and the full pattern of marginal revenues. If marginal revenues are less than one for all activities, then managers are not seeking to maximize earnings from any of them and instead seek to maximize gross revenues (Hypothesis 2a). If marginal revenues are one for spending on all activities that generate revenues, then managers seek to maximize earnings that can be used to cross-subsidize activities which do not generate revenues (Hypothesis 2b). If marginal revenues are at least one for some activities that generate revenues and positive but less than one for other activities, such as selective incentives and information, then managers seek to maximize their influence on collective goals through a variety of direct and indirect channels (Hypothesis 2c).

In addition, I have the following subsidiary hypotheses regarding revenue functions:

HYPOTHESIS 3: *The interaction between spending on selective incentives and information and spending on public goods has a significant effect on environmental citizen group revenues.*

HYPOTHESIS 4: *Revenue function coefficients for spending by environmental citizen groups are contingent on internal structure and the mix of benefits provided.*

HYPOTHESIS 5: *Revenue function coefficients for spending by environmental citizen groups are contingent on external conditions affecting the demand for collective goals.*

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[11] The log-log model takes the form $\ln Y = \alpha^* \ln X$, where α is a vector of coefficients and interaction terms. The marginal effect of X on Y is $dY/dX = \alpha^* Y/X$, so $\alpha = (dY/Y)/(dX/X)$. The second derivative is $d^2Y/dX^2 = (\alpha^2 - \alpha)^* Y/X^2$, so the marginal effect of X on Y is zero or diminishing for $\alpha < 1$, constant for $\alpha = 1$, and increasing for $\alpha > 1$.

[12] I found no studies that differentiate between the "prices" of different kinds of program activities. Some include a marginal tax rate in the price of contributing, since donations to many nonprofit organizations are deductible. In fact, private foundations and government agencies are themselves tax exempt, and many individual members do not itemize.

TABLE 2. Revenues as a Function of Spending and Other Group Characteristics

Log-log Specification		Program Spending Measured in Ratios	
Ln(Per-capita disposable personal income$_t$)	.466 (.386)	Ln(Per-capita disposable personal income$_t$)	1.938* (.551)
Ln(Public goods$_{t-1}$)	.974* (.312)	Public goods$_{t-1}$ ÷ Total spending$_{t-1}$	1.987* (.705)
X 1981–90^a	−.066* (.024)	X 1981–90^a	−.154 (.103)
Ln(Selective incentives and information$_{t-1}$)	1.194* (.262)	Selective incentives and information$_{t-1}$ ÷ Total spending$_{t-1}$	2.952* (.482)
X 1981–90^a	.066* (.023)	X 1981–90^a	.089 (.085)
X No dues^b	−.240* (.065)	X No dues^b	.604 (.928)
Ln(Public goods$_{t-1}$) × Ln(Selective incentives and information$_{t-1}$)	−.056* (.020)	Public goods ratio$_{t-1}$ × Selective incentives and information ratio$_{t-1}$	−3.791 (2.127)
Ln(Fund-raising and recruiting$_t$)	.299* (.042)	Ln(Fund-raising and recruiting$_t$)	.388* (.072)
X No dues^b	.310* (.067)	X No dues^b	.356* (.081)
Ln(Administration$_{t-1}$)	.058 (.041)	Ln(Administration$_{t-1}$)	.145* (.049)
Adjusted R^2	.993		.989

Note: Number of cases = 111. The dependent variable is the natural log of revenues from dues, contributions, gifts, and grants. All variables shown are measured in constant (1992) dollars. Fixed effect coefficients for each group are not shown. Numbers in parentheses are heteroskedastic-consistent standard errors.

$^*p < .01$ (two-tailed test).

[a]"X 1981–90" indicates interaction with dummy variable coded 1 for these years only.

[b]"X No dues" indicates interaction with dummy variable coded 1 for groups that do not distinguish between dues and gifts.

Hypothesis 3 states that public goods and selective incentives and information are not completely independent activities when it comes to generating revenues. Hypothesis 4 states that the numerical values of the empirical parameters defining a revenue function should vary with group type. In practice, I test for differences between groups that distinguish member dues from contributions and those that do not. Hypotheses 5 states that these same empirical parameters should also vary according to external conditions that affect demand for collective goals. Note that hypotheses 4 and 5 refer to differences in the numerical values of particular parameters and do not necessarily imply that managers' preferences for maximizing budgets or some other objective should vary by group type or time period.

RESULTS

Revenue Functions

Table 2 shows the estimates obtained for two different specifications using the natural log of real revenues from dues, contributions, gifts, and grants as the dependent variable. The left-hand side of Table 2 shows the results with all spending variables measured by natural logs. Recall that the coefficients may be interpreted as the percentage change in revenues due to a 1% increase in spending. The right-hand side shows the results using the ratios of spending on public goods or selective incentives and information to all spending. I included a separate intercept for each group in order to account for fixed factors specific to that group. The coefficients for these variables are not shown. In order to test hypotheses 4 and 5, I initially estimated separate coefficients for each spending category (1) depending on whether the group reports member dues and (2) for 1981–90. I then eliminated those interaction coefficients with t-ratios less than one in both specifications and reestimated the model.

Both specifications explain about the same proportion of the variance in the dependent variable. The specification using program spending ratios has much higher and statistically significant coefficients for disposable personal income and administrative spending. The difference in coefficients for the latter is explained by the high correlation between administrative spending and program spending in the log-log specification.[13]

Given the interaction term, both the coefficient and its standard error for the full effect of spending on public goods are contingent on selective incentives and information, and vice versa (Friedrich 1982). The effect of spending on public goods and its standard error also depend on the year, while the effect of spending on selective incentives and information and its standard error depend on both year and group type. These

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[13] I reestimated the equations in Table 2 without a variable for spending on administration, and the revenue functions and implied marginal revenues were qualitatively the same.

TABLE 3. Marginal Revenues from Spending on Different Activities

Activity	Public Goods	Selective Incentives and Information	Fund-raising and Recruiting	Administration
Full sample	.15, .32	.64, .88	1.54, 1.96	.48, 1.22
(*N* = 111)	(.16, .23)	(.11, .17)	(.16, .24)	(.34, .41)
1981–90	.08, .24	.71, .92	1.36, 1.73	.42, 1.05
(*N* = 54)	(.20, .26)	(.11, .15)	(.15, .23)	(.30, .35)
Other years	.20, .38	.56, .84	1.74, 2.20	.56, 1.41
(*N* = 57)	(.13, .20)	(.11, .19)	(.18, .25)	(.40, .47)
Groups that do not report dues	.17, .53	.31, 1.06	4.34, 5.30	.96, 2.40
(*N* = 27)	(.12, .20)	(.16, .35)	(.46, .38)	(.68, .80)
Groups that report dues	.15, .22	.68, .82	1.05, 1.37	.40, 1.01
(*N* = 84)	(.19, .26)	(.11, .17)	(.15, .25)	(.29, .34)

Note: The first number in each column was calculated using the coefficients from the log-log specification on the left-hand side of Table 2. The second number was calculated using the coefficients from the specification on the right-hand side of Table 2, with program spending measured as ratios. Figures in parentheses are the corresponding standard errors. Marginal revenues and standard errors were calculated by setting all independent variables equal to their sample or subsample means.

contingent effects cannot be read directly from Table 2 but are readily computed, given values for the other independent variables.

If I set each independent variable equal to its sample mean, then the contingent coefficient for lagged spending on public goods in the left-hand specification is .064. That is, a 1% increase in spending on public goods yields a percentage increase of only .064 in next year's revenues from dues, contributions, gifts, and grants. The standard error contingent on setting other independent variables equal to their sample means is .068, so the effect is not significantly different from zero. The contingent coefficient for the ratio of lagged spending on public goods to lagged total spending in the right-hand specification is .594 when evaluated at the sample mean, and its standard error is .429.

While neither specification provides support for a direct causal connection between spending on public goods and revenues (Hypothesis 1), this does not mean that spending on public goods is completely irrelevant. I find that the effect of spending on selective incentives and information is contingent on spending on public goods (Hypothesis 3), although this is not significant at conventional levels when program spending is measured by ratios. The negative sign on the interaction term means that we must be careful about making inferences from extreme values, since the predicted change in revenues from increased spending on one kind of activity is negative if we assume that spending on the other is sufficiently high.

The coefficients for spending on selective incentives and information and fund-raising clearly vary by group type when spending is measured by natural logs (Hypothesis 4), although I did not find a significant difference for spending on public goods. The percentage change in revenues due to a 1% increase in spending on selective incentives and information is smaller for groups that do not report dues as a separate revenue source. The percentage change in revenues due to a 1% increase in spending on fund-raising and recruiting is higher for these same groups. When program spending is measured by ratios, the effect of group type disappears with respect to spending on selective incentives and information, but the coefficients are still significantly different for fund-raising.

The revenues of environmental citizen groups also depend on external economic and political conditions. Disposable personal income has a positive effect on revenues, although the coefficient is significant only when program spending is measured by ratios. The log-log specification implies that the elasticity of revenues with respect to lagged spending on public goods was lower during 1981–90, while the elasticity with respect to lagged spending on selective incentives and information was higher (Hypothesis 5). The coefficients on the interaction terms in the right-hand specification have the same signs and exceed their standard errors, but they are not statistically significant at conventional levels. I failed to find a significant difference with respect to spending on fund-raising for either specification.

Marginal Revenues

Table 3 shows the implied marginal revenues from spending on different activities for my full sample and different subsamples. The proportion of cases drawn from each group or period is not exactly the same for each subsample, but I cannot reject the null hypothesis that the cases are distributed randomly.[14] The figures in parentheses are the contingent standard errors for the implied marginal revenues. Since my revenue functions are not linear, marginal revenues depend on both the estimated coefficients and the actual spending levels.[15]

With each independent variable set equal to its full sample mean, I estimate that the last dollar spent on public goods generates only about 15–32 cents in revenues from dues, contributions, gifts, and grants. Implied marginal revenues vary by specification and subsample but never exceed 53 cents. Moreover, the standard errors indicate that the marginal revenues from spending on public goods are generally not significantly different from zero.

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[14] Ten of 54 cases occurring during 1981–90 are for groups that do not distinguish between dues and gifts, compared to 17 of 57 in other years. The chi-square statistic with one degree of freedom is 1.93, which is less than the .90 critical value of 2.71.

[15] The marginal effect of X on Y in the log-log specification is shown in note 11. The specification that measures program spending in ratios takes the form $\ln(Y) = \alpha * X/(X + Z)$, where X is the amount spent on public goods or selective incentives and information, Z includes all other spending, and α is a vector of coefficients and interactive terms. Totally differentiating gives $dY/Y = \alpha * dX/(X + Z) - \alpha * X * dX/(X + Z)^2$. Rearranging terms gives $dY/dX = \alpha * Y * Z/(X + Z)^2$.

Implied marginal revenues from fund-raising are always at least one dollar, and they exceed one dollar for the full sample and every subsample except groups that report member dues as a separate revenue source. These results are clearly inconsistent with revenue maximization (Hypothesis 2a). Implied marginal revenues from spending on selective incentives and information range from 64 to 88 cents for the full sample. Marginal revenues from spending on selective incentives and information are significantly less than one for the log-log specification, which implies that managers seek to maximize resources for pursuing collective goals (Hypothesis 2c). I cannot reject the null hypothesis that managers seek to maximize spending on public goods only (Hypothesis 2b) for the alternative specification.

Marginal revenues from spending on administration differ by specification. For the log-log specification, they are not significantly greater than zero. When program spending is measured by ratios, however, they are significant and greater than or equal to one. As suggested above, the latter result is probably spurious, due to the correlation between spending on administration and spending on programs. A finding of no statistically significant effect is consistent with all three hypotheses, whereas a finding that marginal revenues are significant and equal to or greater than one is inconsistent with revenue maximization.

Differences between group types and shifts in external conditions are also reflected in Table 3, but the pattern of marginal revenues generally does not vary by group type or period. There is one exception, which is that marginal revenues from spending on public goods are significantly different from zero for groups that do not distinguish between dues and contributions.

I explored the importance of different revenue sources further by estimating separate equations for dues or contributions, gifts, and grants for the eleven groups in my sample that do make a distinction. Member dues provide the purest test of Olson's hypothesis that revenues for groups made up of many autonomous individuals will be unrelated to the production of public goods. While some of these individuals make substantial contributions in addition to dues, most members do not have any appreciable effect on the quantity or quality of any public goods. In contrast, contributions, gifts, and grants may come from institutions and large contributors who are not subject to the free-riding logic, because they can make an appreciable difference all by themselves.

Using a log-log specification, I find that the marginal change in member dues from spending on public goods is −.30, with a standard error of .16. If program spending is measured by ratios, the marginal increase in dues is −.33, with a standard error of .17.[16] Estimated marginal increases in contributions, gifts, and grants (and their standard errors) are .32 (.11) for the log-log specification and .44 (.17) when program spending is measured in ratios.

The results for my quadratic specification show a very similar pattern. Marginal revenues from spending on public goods are consistently significant across subsamples, but with one exception they are less than marginal revenues from selective incentives and information. When I estimate separate effects on dues or contributions, gifts, and grants, the marginal increase in member dues from spending on public goods is zero.

DISCUSSION

Studies of collection action have tended to focus on individual motivations for participation. Moe (1981) argues that most other questions about interest mobilization and internal group politics can be resolved by learning why individual members join, but the literature does not provide a definitive answer to this question (Johnson 1995). Moreover, many citizen groups receive a substantial share of revenues from large contributors and institutional patrons. By examining collective action from the standpoint of citizen group managers, I have tested alternative hypotheses about managers' objectives and investigated whether managers must use selective incentives to generate *enough* revenues to maintain financial solvency while pursuing those objectives.

My estimated revenue functions suggest that Olson's theory of cross-subsidies is broadly correct but too simplified. Depending on the functional form, subsample, and revenue source, some of my results reject the null hypothesis that spending on public goods does not lead to increased revenues. Moreover, the interaction of spending on public goods and selective incentives and information is statistically significant. Still, marginal revenues from spending on public goods are always less than marginal cost, and none of my results suggest that spending on public goods leads to a statistically significant increase in revenues from member dues, given spending on other activities.

With respect to managers' objectives, I find that marginal revenues from fund-raising and recruiting are equal to or greater than one for every specification and subsample, which is clearly inconsistent with the hypothesis that managers maximize revenues. Marginal revenues from fund-raising and recruiting are significantly greater than one for some groups, which suggests that some managers actually spend "too little" on these activities. They simply may dislike devoting effort and resources to fund-raising rather than programs to which they are personally committed (see James 1983), or they may have insufficient funds available to invest in raising money.

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[16] These specifications are identical to those in Table 2, except that I omitted any terms for group type and included a term for lagged revenues from contributions, gifts, and grants in the dues equation, and vice versa. This is to control for the possibility that revenue streams are not completely independent (see Richer 1995).

Estimated marginal revenues are most consistent with some version of commitment theory, that is, managers either maximize spending on public goods or maximize net resources available to pursue collective goals. While the ultimate objective is probably influence on public policy or improvement of the environment, spending on public goods produced by the group itself is instrumental to achieving these objectives. Marginal revenues from spending on selective incentives and information suggest that this also yields positive marginal benefits, although the strength of this finding varies somewhat by specification.

Of course, the cases in my sample do not encompass all types of interest groups. It remains to be seen whether an analysis of financial data for more traditional economic groups would support some other hypothesis. I do find that groups which do not distinguish between member dues and contributions have different revenue functions and allocate their resources differently than do other groups, and revenue functions for environmental citizen groups did shift during the 1980s. I find little evidence, however, that managers' objectives vary by group type or time period. This conclusion must be qualified by the recognition that possible objectives have been defined broadly and that the groups in my sample produce a variety of different public goods and seek influence in a variety of different ways.

Although I lack direct measures of member or patron motivations and managerial autonomy, my evidence sheds some light on these issues. My results suggest that at least some forms of financial support for collective action do not depend solely on selective incentives. Moreover, the selective incentives provided by groups in my sample are primarily publications and group activities that also provide information about environmental issues and build support for the collective goals of the organization. The key to organizational maintenance by managers of environmental citizen groups appears to be the joint production of public goods, complementary selective incentives, and information.

My results also imply that managerial autonomy is limited by the fact that revenues respond to resource allocations, but different revenue sources respond differently. Therefore, managers who want to alter significantly their resource allocation patterns must alter their mix of revenue sources.

Finally, my finding that managers seek to maximize resources available for the pursuit of collective goals is also consistent with rational, self-interested behavior. Managers who have at least some control over the allocation of resources for an entire organization are able to make an appreciable difference in collective outcomes by their efforts alone. Rather than being preoccupied with maximizing the budget and maintaining the organization, managers of environmental citizen groups appear to be rationally committed to influencing public policy and improving the environment, subject to the constraint imposed by the need for financial solvency.

APPENDIX A: THE REDUCED FORM MEMBERSHIP FUNCTION

Is it reasonable to assume that the second partial derivatives of membership with respect to total spending are negative, given that spending on selective incentives is actually a function of total members? To make the discussion tractable, I adopt specific functional forms for these expressions. For the sake of simplicity, I limit the model to two activities.

Assume that membership is a function of member dues (P), a quality index of selective incentives supplied to all members (Φ), and perhaps spending on public goods (Q_1). Spending on selective incentives (Q_2) depends on quality, total membership, and perhaps spending on public goods, if there are economies of scope from joint production. For example, the cost of producing those portions of *Sierra* magazine devoted to reporting on political developments may be affected by the fact that the Sierra Club also engages in lobbying activities.

Assume further that both functions have a Cobb-Douglas form. Then we have:

$$M = P^{\alpha1}\Phi^{\alpha2}Q_1{}^{\alpha3} \qquad \textbf{(A-1)}$$

$$Q_2 = \Phi^{\beta1}M^{\beta2}Q_1{}^{\beta3} \qquad \textbf{(A-2)}$$

Membership is a decreasing function of price and an increasing function of quality but with diminishing marginal returns, so $\alpha1 < 0$, $0 < \alpha2 < 1$. Membership is either unaffected by public goods or is positively affected, but with diminishing marginal returns, so $0 \leq \alpha3 < 1$. Q_2 increases with respect to both the quality of selective incentives and total membership. It is either unaffected by or decreases with spending on public goods, depending on whether there are economies of scope. Therefore, $0 < \beta1$, $\beta2$, but $\beta3 \leq 0$.

Solving for the quality index by taking the inverse of A2 gives:

$$\Phi = Q_2{}^{1/\beta1}M^{-\beta2/\beta1}Q_1{}^{-\beta3/\beta1} \qquad \textbf{(A-3)}$$

Substituting A-3 into A-1 and rearranging terms gives:

$$M = P^{\pi1}Q_1{}^{\pi2}Q_2{}^{\pi3}, \qquad \textbf{(A-4)}$$

where $\pi1 = \alpha1^*\beta1/(\beta1 + \alpha2^*\beta2)$, $\pi2 = (\alpha3^*\beta1 - \alpha2^*\beta3)/(\beta1 + \alpha2^*\beta2)$, $\pi3 = \alpha2/(\beta1 + \alpha2^*\beta2)$. This expression corresponds to the generalized, reduced-form membership function in the text.

Note that my assumptions imply $\pi1 < 0$, $\pi2 \geq 0$, $\pi3 > 0$. Thus, membership decreases when price increases, is unaffected by or increases with spending on public goods, and increases with total spending on selective incentives. The second partial derivatives with respect to Q_1 and Q_2 are negative if and only if $\pi2$, $\pi3 < 1$. For $\pi2$ this requires $(\beta1^*(\alpha3 - 1) - \alpha2^*\beta2)/\alpha2 < \beta3$. The left-hand side of this inequality is always negative, so the condition is satisfied if $\beta3$ is not even more negative, that is, if any economies of scope are not too great. For $\pi3$, we need $\alpha2 < \beta1 + \alpha2^*\beta2$, or $\alpha2 < \beta1/(1 - \beta2)$. This condition is satisfied if spending on selective incentives exhibits constant or decreasing returns to scale from either quality or members, so that either $\beta1 \geq 1$ or $\beta2 \geq 1$. Even if both $\beta1$ and $\beta2$ are less than one, the condition is still satisfied if they are not too small relative to the effect of quality on membership.

APPENDIX B: CLASSIFICATION OF SPENDING DATA

Spending data were classified according to four categories: public goods, selective incentives and information, fund-raising and recruiting, and administration. Fund-raising and recruiting includes expenditures labeled "member development," while administration includes amounts for the board of directors or regional offices. Table B-1 shows the categories as they appear in annual reports and tax documents that were assigned to public goods or selective incentives and information. "Public goods" includes attempts to influence public policy (including litigation), scientific research, and private activities that affect environmental quality, such as wildlife management programs. "Selective incentives and information," which includes member benefits and communications, refers to attempts to influence public awareness of environmental issues and problems.

Spending figures for selective incentives provided to members only and activities directed at a broader public are often combined in the source documents. For example, the Sierra Club divides its program spending into "Studying and Influencing Public Policy," "Information and Education," and "Chapter Allocations," plus outdoor activities that generate separate fees and that I treat as net revenues from sales and events. The information and education category includes the cost of *Sierra* magazine, distributed to all members, as well as the literary programs of Sierra Books (Sierra Club 1994a). Therefore, I include both "Information and Education" and "Chapter Allocations" in my category of selective incentives and information.

I adjusted spending for two groups by subtracting spending on activities provided only upon payment of a separate fee to the general public and then including the net total on the revenue side under "net sales and events." For the National Wildlife Federation, I subtracted amounts spent on producing and promoting educational materials; for the Sierra Club, I subtracted the cost of outdoor travel trips. These adjustments were necessary in order to have consistent categories, since most groups simply report net earnings from sales and special events on the revenue side of the balance sheet. Sierra Club trips can be considered a selective incentive, since they technically are available only to members, but the trips cost several hundred to several thousand dollars (compared to annual dues in 1994 of $35), and membership applications are accepted as part of the trip reservation (Sierra Club 1994b). Revenues from trip fees in 1993 were $2.8 million. Assuming a mean fee of $500, this implies that only about 1% of Sierra Club members went on the trips.

The National Parks and Conservation Association's 1987 Annual Report includes only one figure for all program activities. Program expenditures were allocated to public goods or selective incentives and information using the average shares for 1986 and 1988. The World Wildlife Fund formally merged with the Conservation Foundation on July 1, 1990. Figures for fiscal 1990 include separate totals for the Conservation Foundation obtained from the Massachusetts attorney general's office.

TABLE B-1. Classification of Program Spending

Public Goods	Selective Incentives and Information
American Forests [1991 AR; 1992–94 Tax and FS]	
Research and policy	Conservation education
Conservation action	
American Rivers, Inc. [1989, 1990, 1992, 1994, 1995 Tax and FS; 1991, 1993 AR]	
River conservation	Public education and information
Hydro power	Member communications
Other programs	
Center for Marine Conservation [1988–91 MA AG; 1992–95 Tax and FS]	
Fisheries	Public education
Habitat protection	Member services
Pollution prevention	
Wildlife conservation	
Species recovery	
Biodiversity	
International initiative	
Defenders of Wildlife [1986–92 AR; 1993–94 Tax and FS]	
Wildlife action	Information and education
	Member communications
Environmental Defense Fund [1985–93 AR; 94 MI AG]	
Energy and air	Education
Toxic chemicals	Membership
Wildlife and water	
Legislative	
Friends of the Earth [1990 Tax; 1991 MA AG; 1992–94 FS]	
Public health	Other and outreach
Natural resources	Members
Global pollution	
Energy	
Domestic programs	
International programs	

Note: In brackets are the years for various source materials: AR = Annual Report; Tax = IRS Form 990; FS = group's financial statement; and "XX" AG = records filed with the attorney general's office in state XX.

TABLE B-1. (Con't.)

Public Goods	Selective Incentives and Information
Izaak Walton League [1987–89 MA AG; 1990–91 FS; 1992 AR]	
General conservation	Members services
Upper Mississippi region	*Outdoor America*
Other programs	
National headquarters	
National Audubon Society [1983–93 AR; 1994 Tax and FS]	
Wildlife conservation	Chapter activities
Science and field research	Members and publications
	Environmental education and information
National Parks and Conservation Association [1979–93 AR; 1994 FS]	
Parks and conservation	Field programs
Urban recreation	Information and education
Immigration	Louisiana Expo
Natural resources	Public education
Cultural resources	Members
Grass roots	
National Park Service and land	
Urban parks and recreation	
Carrying capacity	
Natural Resources Defense Council [1988 MA AG; 1989, 1990, 1993 AR; 1991, 1992, 1994 FS]	
Environmental programs	Public education
Science	Member services
Legislative	
Intern program	
National Wildlife Federation [1989–92 AR; 1993–94 FS]	
Other programs	Associate members
	Ranger Rick
	Big Backyard
Sierra Club [1975–93 AR]	
Public policy	Information and education
	Chapters
Sierra Club Legal Defense Fund [1989–91 MA AG; 1992–95 Tax and FS]	
Programs	Public information
Trout Unlimited [1990–91 FS; 1992–94 Tax]	
Resources	Communications
	Chapters
Wilderness Society [1981–89 AR; 1990–94 FS]	
Conservation projects	Public education
	Membership
World Wildlife Fund [1990–92 FS; 1993–94 AR]	
Less-developed countries	Members
Developed countries	Public information

Note: In brackets are the years for various source materials: AR = Annual Report; Tax = IRS Form 990; FS = group's financial statement; and "XX" AG = records filed with the attorney general's office in state XX.

APPENDIX C: ILLUSTRATIVE FINANCIAL DATA FOR GROUPS IN THE DATA SET

TABLE C-1. Percentage Distribution of Revenues

Group	Revenue ($000)	Dues	Contributions, Gifts, and Grants	Sales and Events	Other
AF	3,564	12%	81%	6%	1%
ARI	1,892	12	84	2	2
CMC	11,861	*	96	0	4
DOW	5,212	26	64	7	9
EDF	20,747	*	95	1	4
FOE	2,759	11	87	1	2
IWL	2,037	40	56	1	3
NAS	43,361	25	39	21	14
NPCA	11,392	52	39	8	1
NRDC	17,602	*	90	1	9
NWF[a]	58,327	55	25	8	12
SC[a]	42,140	37	34	10	19
SCLDF	9,483	*	83	14	3
TU	3,026	50	24	20	5
WS	16,641	36	56	0	7
WWF	55,669	*	91	0	9

Source: Author's calculations from annual reports, tax returns, and financial statements.

Note: Figures are for calendar 1992. Percentages sum across rows and may not add to 100 due to rounding.

*Group does not report member dues as a separate revenue source. For abbreviations, refer to text Table 1.

[a] Net earnings from education materials sold by the National Wildlife Federation and from Sierra Club outings are included in sales and events. See Appendix B.

TABLE C-2. Percentage Distribution of Expenditures

Group	Total ($000)	Public Goods	Selective Incentives and Information	Fund-raising and Recruiting	Administration
AF	3,712	38%	39%	10%	13%
ARI	1,890	56	26	12	6
CMC	6,295	51	20	24	6
DOW	5,606	40	25	26	9
EDF	20,132	60	20	16	4
FOE	2,548	62	19	8	11
IWL	2,075	57	20	8	15
NAS	43,437	20	47	22	9
NPCA	10,713	13	62	22	3
NRDC	16,868	53	25	12	10
NWF[a]	56,126	35	33	24	8
SC[a]	40,395	29	33	29	10
SCLDF	9,314	70	7	17	6
TU	2,751	25	46	17	12
WS	16,484	32	43	15	10
WWF	56,170	55	34	7	4

Source: Author's calculations from annual reports, tax returns, and financial statements.

Note: Figures are for calendar 1992. Percentages sum across rows and may not add to 100 due to rounding. For abbreviations, refer to text Table 1.

[a] Spending on educational materials sold by the National Wildlife Federation and on Sierra Club outings have been incorporated into net revenues from sales and events. See Appendix B.

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The Strategic Timing of Position Taking in Congress: A Study of the North American Free Trade Agreement

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A critical element of decision making is the timing of choices political actors make; often when a decision is made is as critical as the decision itself. We posit a dynamic model of strategic position announcement based on signaling theories of legislative politics. We suggest that members who receive clear signals from constituents, interest groups, and policy leaders will announce their positions earlier. Those with conflicting signals will seek more information, delaying their announcement. We test several expectations by examining data on when members of the House of Representatives announced their positions on the North American Free Trade Agreement. We also contrast the timing model with a vote model, and find that there are meaningful differences between the factors influencing the timing of position announcements and vote choice. Our research allows analysts to interpret the process leading up to the House action and the end state of that process.

The countless strategic calculations of political actors necessarily involve timing, which is an inherently dynamic process. The more momentous the decision, the more important the issues of timing are likely to be. For example, it is widely understood that the timing of elections is critical to the ability of a ruling party to maintain its majority status (Alt and King 1994, King et al. 1990, Warwick 1992). Presidents time major speeches and trips so as best to influence their chances for reelection (Brace and Hinckley 1993). Similarly, justices of the Supreme Court (Hagle 1993), as well as judges of the U.S. Courts of Appeals (Spriggs and Wahlbeck 1995) and U.S. District Courts (Barrow and Zuk 1990) have been shown to time their retirement so as to increase the odds of a like-minded successor being appointed.

Timing is also important in the U.S. Congress, as a few examples illustrate. When a vote is taken can mean the difference between passage and failure, a quick and decisive position announcement by an influential member can affect the vote decisions of many of his or her colleagues, and the timing of critical announcements during a campaign can mean the difference between election and defeat. As a result, strategic calculations, such as voting decisions, have both cross-sectional and temporal dimensions. But while almost all politics is dynamic, much political analysis is static. Examinations of congressional voting behavior, for example, have failed to focus on the matter of timing.

We use duration analysis, specifically Cox's (1972) proportional hazards model, to illustrate the possibilities of dynamic analysis. We examine the U.S. House vote on the North American Free Trade Agreement (NAFTA) Implementation Act (H.R. 3450) of November 17, 1993, and the timing of members' decisions on that vote. It has been called "the most important vote on Capitol Hill since the Berlin Wall came down," and it "gave President Clinton the most significant policy success of his first year" in office (Frenzel 1994, 3). The NAFTA debate figured prominently in media broadcasts before its implementing legislation was to be proposed to the House, and members of Congress announced their vote intentions as early as 15 months beforehand. The strong pressures coming from constituencies and interest groups, coupled with the overall salience of NAFTA in national politics, make it appropriate for analyzing the importance of timing in political decision making.

Our primary objective is to develop a dynamic model of the strategic timing of position taking by members of the U.S. Congress. In so doing, we draw upon the roll-call literature and, more specifically, Krehbiel's (1991) use of signaling models. Because position taking reflects legislators' desires both as signal senders and receivers, examining their behavior over time provides both a clearer indication of the effect of such signals on congressional behavior and a more complete picture of legislative decision making. Our model allows analysts to interpret both the process leading up to the House action and the end state of that process, and it permits comparisons between the determinants of two conceptually different aspects of voting.

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STRATEGIC TIMING

Studies of congressional decision making have focused largely on cross-sectional analyses of roll-call votes, ignoring the timing dimension in legislative formulation (Baron 1990, Lindsay 1991). Even relatively sophisticated models of roll-call voting have not moved us beyond focusing on the end of the legislative process (see, e.g., Bartels 1991). While such research provides a wealth of useful information about decision making in Congress, a serious problem is that "roll call votes are not a random sample of Congressional decisions" (VanDoren 1990, 311). Studying strategic position taking and the timing of voting decisions begins to address problems of a lack of randomness by not focusing strictly on the end of the process, that is, the roll-call vote. Members of Congress clearly make voting decisions at various points in the legislative process. Attention to timing thus adds information, specifically sequence, to such analyses. In addition, the inclusion of the timing dimension more closely approximates members' strategic considerations and therefore provides a more complete picture of legislative voting behavior.

Legislative signaling theory offers a solid basis on which to draw out our predictions for the strategic timing of position announcements. Krehbiel (1991) points out that although signaling models are relatively new to political science, the behavior that they characterize has been described for a long time. The tie between signaling models and traditional descriptions of voting behavior is straightforward. Both are based on the premise that "legislators need information about policy choices they must confront but may not understand. Therefore, legislators devise strategies to cope with uncertainty. Taking cues from other, relatively informed legislators is conceptually the same as receiving signals" (p. 70). Of the many roll-call models in the literature, the cue-taking models used by Kingdon (1989) and Matthews and Stimson (1975) are the most closely related to signaling models. Krehbiel (1991, 70) also points out that Buchanan et al. (1960) describe members as judging alternatives in light of their own or their constituents' values.

This dynamic process of sending and receiving signals among representatives and their constituents and colleagues occurs during the weeks and months preceding the final vote. Legislators engaged in this process are concerned not only with *how* they vote but also with *when* they announce their position on an issue. Like the vote itself, the strategic timing of a position announcement is important because it can either facilitate or hinder a member's pursuit of electoral and policy goals.[1] In other words, there are potential costs and benefits of making position announcements earlier or later in the debate on a particular legislative item. A member will weigh the costs and benefits of the announcement timing in light of the degree of consensus among the signals s/he receives and time the position announcement accordingly.

From a member's perspective, early decisions may help facilitate goals. By announcing a position early in the debate, the member lets constituents or interest groups know that s/he recognizes their importance. This is a validation for those who support the position because they see that the representative takes their preference seriously. Such recognition may enhance electoral support in the future. Early position announcements can mobilize favorable constituency elements. The member's early position taking facilitates an electoral commitment to the member. Also, an early announcement may enhance the role of a policy entrepreneur. Members who are willing to enter the fray earlier may have a greater opportunity to influence the development of the legislation. The announcement may come with stipulations, but by getting out in front of an issue, members signal their intention to participate in the debate.[2] In addition, certain institutional positions, such as relevant committee membership, may facilitate participation. By serving on the committee of jurisdiction, members have the opportunity to consider legislation formally earlier in the process than do those who do not serve on the committee. Finally, early announcements may enhance a member's chance of influencing others, which facilitates the goal of achieving intra-Washington influence and, therefore, making good policy. By announcing early, members provide cues for others to follow. If a member who wishes to lead on an issue waits too long to take a position, then the pool of legislators to be influenced diminishes as other members announce their position.

Early announcements are not without potential costs, however. The electoral benefits of early position taking depend on a member's ability to read preferences accurately. If a member chooses the "wrong" position in terms of constituency or interest group preferences, then s/he has only two options. First, s/he may change position, which is a costly endeavor. Those who support the position the member deserted are certain to be unhappy. At the same time, supporters of the new position, while pleased with the decision, will be somewhat skeptical about the member's commitment. In addition, a position change leaves the member open to charges of "flip-flopping," an unpopular label (Fenno 1990). Second, the representative may stick with the original position as announced. In this case, s/he avoids appearing indecisive but may suffer severe electoral consequences because s/he alienates an important segment of the constituency. This suggests a second possible cost to early position taking: Just as early announcements may mobilize groups favorable to the position, they also may mobilize groups who oppose the position. Magnifying this potential cost is the likelihood that opposition groups can be more damaging to the member's electoral fortunes than supporting groups can be helpful (Fiorina 1975).

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[1] For a discussion of member goals, see Kingdon (1989).

[2] Fenno (1990) argues that members may not make an early commitment in order to preserve their flexibility in the policy development process. By keeping their options open, they remain a part of the negotiating process. This interpretation does not require a member to withhold an early position announcement, however. Instead, to be part of the negotiations, actors must have at least an initial preference. Negotiation assumes people with opposing viewpoints work to find alternatives that meet the minimal requirements of both sides. It is inadvisable to assume that those who wish to participate in policy development will enter the debate with no announced position. Instead, they are likely to enter the debate with a starting preference while signaling their willingness to make some concessions.

Furthermore, late position announcements may have some potential benefits. Most important, if representatives perceive that the competing constituent interests in their district are closely matched, then late announcements give them time to collect more information about the electoral and policy ramifications of their choice. Members who wait to take a position also may receive side benefits from the president or legislative leaders in exchange for their vote.

The costs of late announcements mirror the benefits of early position taking. By failing to send a validating signal to their constituents, members who wait to announce their vote intention may alienate (rather than mobilize) otherwise supportive groups, resulting in a loss of electoral enthusiasm or even support. Members may lose an opportunity to participate in the formulation of policy or to influence other members. As a latecomer to the fray, their credibility on the issue may be diminished (Fenno 1990). Finally, in a close vote, unwanted attention may be focused on a member, especially when the vote ultimately cast is not popular.

The cost-benefit calculus that drives the timing of position taking thus hinges on the degree to which the different signals are both strong and unambiguous. As suggested by Kingdon (1989), the strength of a signal is largely a function of an issue's salience to the member's constituency. High-salience issues, those in which the constituency has a vital interest, will send strong signals to the member about expectations for his or her vote. In contrast, low-salience issues make it more difficult for a member to obtain information about constituent preferences. Thus, issue salience has a direct bearing on the strength (albeit not necessarily the clarity) of constituency signal a member receives.[3]

In addition to the strength of signal, a member also must consider the signal's content; specifically, whether constituents are unified or divided on the issue at hand. Thus, when considering constituency interest, a member may perceive one of two possible signals. First, there may be a clear choice that coincides with strong constituent interests; we expect this member to announce early.[4] S/he has no doubt about which position to take and thus has no need to collect more information on the proper course of action. By taking a position early, the member also sends a signal to the constituency about commitment to their position. Second, the member may perceive mixed signals about which choice more closely coincides with constituent interests. Relatively balanced interests within the district make it difficult to identify the most beneficial position. Under such circumstances, a member will wish to collect more information from both constituents and colleagues before finalizing a decision, making a delay in position taking attractive and even necessary.

In summary, when the signals are compatible, we expect members to take a position early. Under these circumstances, the benefits of an early announcement are clear, and the costs are minimized. In addition, because the wishes of constituents are unambiguous, a member may reap the advantages—ability to influence both the content of the legislation and the decisions of other members—without any of the associated costs. In game-theoretic terms, the representative can serve as a signal *sender*, both to constituents and to other legislators. If constituency demands are ambiguous or mixed, or if those demands conflict with the member's conception of good policy, then we expect members to take a position later in the process. Uncertainty about the correct choice means members will take longer to gather information and weigh the severity of the costs incurred when a position is taken. Such members are signal *receivers*, for in taking a position they will inevitably alienate important constituency segments and interest groups or betray their own sense of good policy.

NAFTA POLITICS

The vote on NAFTA is used to clarify and illustrate the model.[5] Quinones (1994) points out that NAFTA is by far the most comprehensive of all free trade agreements considered by Congress. Its main thrust was to eliminate tariffs on goods sold and produced in North America. President Clinton argued that the outcome of the vote would define the credibility of the United States in all subsequent trade negotiations, including the Uruguay Round of the General Agreement on Tariffs and Trade (GATT). Such rhetoric raised the stakes beyond NAFTA itself.

Much of the debate appeared to pit members who favored free trade against those who supported organized labor. The bulk of the evidence suggested that NAFTA would result in a net gain of employment in the long run, but both sides agreed that short-term adjustments would result in a loss of jobs in some industries (Quinones 1994). In addition, environmentalists feared industry would take advantage of less rigorous pollution regulations in Mexico.

The politics of NAFTA increased the salience of the issue. President Clinton campaigned as a "new" Democrat, and in facing off against organized labor, he could establish credibility as someone willing to move beyond his party's traditional coalition. Another political factor bringing NAFTA to the fore was the role of Ross Perot. After his strong showing in the 1992 presidential election, Perot attempted to use NAFTA as a vehicle to flex his political muscle and show that he and his followers would be a force in the 1994 elections. While he was less than successful in this attempt, due partly to his poor performance in a debate on the agreement with Vice President Gore, Perot's emphasis on the issue helped keep it in the news and on people's mind. Members of Congress and government officials participated in and carefully monitored the NAFTA debate.

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[3] Sinclair (1982, 12) concurs; if there is opinion (or signal) conflict, "the member's voting behavior should depend upon the saliency of the issue. If the issue is highly salient to the reelection constituency, she or he should vote in accordance with this segment's opinions. Although the representative needs contributions and campaign workers, votes win elections. If, however, the issue is salient only to the supportive elite, one would expect their opinions to be expressed in the member's vote.... The level of party cohesion, then, is determined by whether members of a party are receiving congruent or conflicting constituency signals."

[4] If constituency interests are clearly one way or the other, it is not reasonable to expect a member to hold out for concessions, since such behavior, in the face of constituency preferences, would not be credible.

[5] This section draws on Quinones (1994). See also Keech and Pak (1995) and Uslaner (1995).

All these factors led us to choose NAFTA as a suitable vote for the analysis.[6] We agree with Krehbiel's (1996) clearly articulated argument about the value of case-specific analyses (like NAFTA) to improve legislative research beyond the case at hand. In particular, case studies are not substitutes for large-*N* studies but can be "*opportunistic complements*" of them (Krehbiel 1996, 3, emphasis in original). Because of NAFTA's high salience, all strategic calculations, including timing, should have received more consideration by members of Congress. If our theory and empirical evidence do not generally hold for the NAFTA case, then the model is unlikely to be useful in exploring other issues.

A MODEL OF THE TIMING OF POSITION ANNOUNCEMENTS AND VOTING

We analyze two aspects of House decision making on NAFTA: the final roll-call vote for implementation and the timing of members' announcements of their vote intention on the implementation legislation.[7] The variables and their hypothesized effects on both the vote and the timing of announcements are explained below. The discussion of each independent variable clarifies why the determinants for the two models are different.

Constituency Factors

Constituency influence involves complicated measurement issues and has been one of the most vexing problems in the voting literature (Kuklinski 1979). How should constituency influence be considered? Is it a matter of policy preferences, as the congruence literature suggests, or of the distributive benefits a constituency might receive? The preceding discussion of NAFTA indicates at least one surrogate for constituency influence: labor union membership. A high rate of unionization in a district suggests opposition to NAFTA. The expected effect of union membership on timing, however, is best conceptualized in terms of the homogeneity of the district. Thus, we expect that representatives with either high or low unionization in their district received clear signals about NAFTA and thus were likely to make their vote decision earlier than colleagues with mixed union representation in their district. We expect the latter, without a clear signal regarding their optimal position on NAFTA, to hedge their position and delay their announcement. *Union Membership* is coded as the percentage of all private-sector workers who belong to a union in the representative's district.[8]

Several other factors also measure constituency influence. First, there is the geographic location of the district. Areas closest to the *Mexican Border* had much to gain from the fall of trade barriers between the two countries, so members from border districts should be more likely to vote in favor of NAFTA.[9] On the question of timing, the salience of NAFTA for such districts should increase the strength of signal a member receives from constituents. As a result, border districts should have a positive effect on the timing of the vote decision, meaning that representatives of such districts would announce earlier than nonborder colleagues.

Second, because Ross Perot was a spokesperson for anti–NAFTA forces, the strength of his support in the 1992 election may have signaled representatives about the degree to which their constituency would support NAFTA. Thus, the model includes a measure of Perot's showing in 1992 (*Perot Vote*). We expect support for Perot to have a negative effect on a member's vote choice. We also anticipate that in districts where Perot did very well or very badly in 1992 the member would receive a clear signal on the potential costs of supporting or opposing the bill, while in those where Perot's support was somewhere in the middle the signal would be unclear. So the effect on the timing of a member's decision is like that of union membership: Low or high Perot support implies an earlier announcement, while median levels indicate delay.

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[6] Although NAFTA was unique in some ways, in its journey through the legislative process it resembles other omnibus bills, on which Congress increasingly relies as vehicles for legislating. One potentially significant way in which NAFTA differed from other legislation was the fast-track procedures used, which limit the time for consideration and preclude amendment or filibustering. The fast track undoubtedly heightened members' awareness of the importance of timing, since it assured that a vote would be forthcoming soon after the implementation bill was introduced. The fast track also affects our theoretical expectations for the effect of committee membership on timing, since committees have limited ability to influence the final form of legislation under that procedure.

[7] Several authors have considered the factors affecting how a House member voted on NAFTA. See Quinones (1994), Uslaner (1995), or Wink, Livingston, and Garand (1996). While the latter incorporate similar theoretical foundations by discussing the role of cross-pressures in the decision-making process, none of these studies examines the timing aspect in decision making.

[8] These data were built by merging disaggregated union membership data at the Metropolitan Statistical Area level with the Bureau of the Census Consolidated Federal Funds Report Geographic Reference File and subsequently mapping to each of the 435 post–1990 Census congressional districts (see Appendix B).

[9] We did not include a variable for districts bordering Canada because NAFTA made only minor changes to the U.S.–Canada Free Trade Agreement (*Congressional Quarterly Almanac* 1993, 171). An alternative operationalization of the variable could include all districts in the states bordering Mexico. Comparing the results obtained using states versus districts bordering Mexico, we find NAFTA to be more salient in the districts than in the states. Members from border districts were among the first to announce their position. When we consider these different operationalizations in the vote choice model, we find that while representing a border district does not help explain a member's vote, representing a district within a border state does. This may be due to the signals received from others in the state delegation, which may influence member voting behavior (Kingdon 1989). Since our focus is on the timing of position announcements for high-salience issues, we give the results for border districts, which more clearly represent this situation. The alternative results can be found with the archived data and statistical output. The percentage of Mexican American citizens within the district (not the percentage Hispanic), although appealing as an indicator of NAFTA's salience, is impractical, since we were unable to obtain data that differentiated the Hispanic population by national origin at the congressional district level.

Finally, the socioeconomic condition of a district may have affected the degree to which constituents favored NAFTA. The threat of job loss due to NAFTA was greatest for low-wage workers. Consequently, members from districts with lower household incomes are expected to be less supportive of the bill. *Household Income* is measured as the district's median figure divided by $10,000. With respect to timing, as for previous variables, high or low values for household income are expected to compel earlier announcements, and values in the middle range to cause delay in taking a position.

Interest Group Factors

We examine two factors associated with interest group influence, campaign donations from corporate political action committees (*Corporate Contributions*) and contributions from labor-related PACs (*Labor Contributions*). We expect members with a large percentage of total contributions from business sources to favor the legislation, while representatives with a large proportion from labor sources should be more likely to vote against NAFTA. We also expect members who receive a large proportion of donations from either source to announce their position earlier, reflecting a clear signal. By doing so, these members would solidify their standing with a group supportive in past elections, or at least avoid alienating that group. Press reports indicate that some labor organizations threatened to withhold support for members in future elections if they voted for NAFTA, demonstrating the electoral ramifications of the issue (Galvin 1993; see also Quinones 1994).

Individual Factors

Party Affiliation is often considered an important cue for members when deciding their position on an issue. In the NAFTA case, the effect is complicated. Party was a fairly direct cue for Republicans, whose congressional leadership and party professionals supported the agreement, which had been negotiated by the Bush administration.[10] Thus, the effect of the party variable for Republicans is positive for the vote model. There was not the same degree of consensus among Democrats. While the president favored the agreement, the congressional leadership was divided.[11] This lack of consensus and the Democrats' historical alliance with labor likely meant that party leaders who supported NAFTA had less leverage over members. Consequently, although we include this indicator in our analysis of the vote, we have no strong expectation about the relationship between party and vote choice. Likewise, we have no strong expectation with respect to the influence of political party on timing of House members' decisions. Rank-and-file party members in Congress do not have the same likelihood of swaying colleagues as do those with institutional stature or acknowledged expertise. Without the motivation of influencing others and absent other factors, such as ideology or constituency factors, there is no reason to expect one party's rank-and-filers to announce earlier than those of the other; so party is excluded from the timing analysis.[12]

Ideology is captured by the vote rating given to the House member by the Chamber of Commerce in 1993.[13] Because NAFTA clearly demonstrated a labor-management division, the Chamber of Commerce or AFL-CIO measure is likely to be the most appropriate of the numerous voting scores available. This measure was purged of the NAFTA vote, which was used in the original calculations of the 1993 scores. A positive relationship between ideology and support for NAFTA is expected.

With regard to timing, we expect that the most ideological members will declare their position early. Those with extreme ideologies have strong policy preferences, which would suggest early action. Since ideology is a proxy for members' policy preferences, which may conflict with constituents' policy preferences, ideology and constituency factors must be considered together.[14] Several authors demonstrate the influence of ideology on voting *after* controlling for constituency interests (Kalt and Zupan 1990, Kau and Rubin 1978, Sinclair 1982). This also suggests that the effects of union membership and household income need to be considered simultaneously with the ideological predisposition of the member to capture the cross-pressures representatives experience.

If a district has an extraordinarily high or low percentage of constituents who belong to a union, the member receives a clear voting signal and will, ceteris paribus, declare a position early; this suggests a qua-

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[10] Some conservatives, such as Duncan Hunter (R-CA) and Helen Delich Bentley (R-MD), urged colleagues to vote "no," saying it was no longer a Republican-Bush initiative. This suggests that an ideological measure may be particularly important.

[11] Speaker Thomas Foley (D-WA) was a supporter, while Majority Leader Richard Gephardt (D-MO) and Majority Whip David Bonior (D-MI) were opponents; Bonior was a leader of the anti-NAFTA forces in Congress.

[12] Inclusion of a party affiliation variable in the model of timing results in a nonsignificant coefficient. The only difference in the other variables is that the effect of labor contributions decreases in significance. This is probably due to colinearity, since labor tends to contribute more to Democrats than Republicans.

[13] Using other voting scores, such as those calculated by the AFL-CIO or ADA, does not alter the results. A graph of ideology by timing yields a U-shape figure, indicating that members with more extreme preferences announce earlier than do those who are more moderate.

[14] Household income and union membership are surrogates for constituent ideology; thus, we consider these variables in tandem with member ideology. In contrast, if Perot did poorly in a district, there is no clear indication about whether the constituency is pro- or anti-NAFTA.

TABLE 1. Hypothesized Effects on Direction and Timing and Summary Statistics

	Hypothesized Effect on					
Variable	Direction	Timing[a]	Mean	Standard Deviation	Min.	Max.
Dependent Variables						
Vote			0.54	0.50	0.00	1.00
Timing of Position Taking			402.86	70.45	1.00	463.00
Constituency Factors						
Union Membership	–	+ if high or low	0.12	0.06	0.02	0.32
Mexican Border	+	+	0.03	0.16	0.00	1.00
Perot Vote	–	+ if high or low	0.19	0.06	0.03	0.33
Household Income	+	+ if high or low	3.07	0.84	1.45	5.72
Interest Group Factors						
Corporate Contributions	+	+	0.15	0.10	0.00	0.42
Labor Contributions	–	+	0.10	0.11	0.00	0.48
Institutional Factors						
NAFTA Committee	Not in model	+	0.30	0.46	0.00	1.00
Democratic Leadership	Not in model	+/–	0.05	0.22	0.00	1.00
Republican Leadership	Not in model	+	0.04	0.20	0.00	1.00
Individual Factors						
Party Affiliation	+/–	Not in model	0.59	0.49	0.00	1.00
Ideology	+	+[b]	0.44	0.50	0.00	1.00

[a] This is the hypothesized effect on the hazard rate; thus, "+" indicates taking a position earlier.
[b] See discussion in text about the interaction between ideology and constituency factors.

dratic relationship between timing and union membership. Yet, the signals themselves are in opposite directions; high unionization gives a signal to oppose NAFTA, while low unionization sends a signal to support it. As a result, once ideology interacts with these variables, the form of the relationship changes. Liberal members with high unionization in their district will declare their position early, while liberals with low unionization in their district receive conflicting signals, that is, their constituency factors suggest voting for NAFTA, while their ideological and policy predispositions suggest voting against it, and thus delaying their announcement. The process for conservatives is similar: Those from low-union districts receive a constituency signal consistent with their personal preference, while those from high-union districts find their preference at odds with that of constituents. The same relationship also holds for household income in the district: Low-income values for liberal members and high ones for conservatives are compatible signals and are consistent with early position taking, while the reverse circumstances indicate contradictory forces that prompt members to delay their decision.

Institutional Factors

We posit that institutional factors may help House members achieve policy goals. Specifically, a powerful institutional position may help a member serve as a cue-giver and therefore influence policy. Yet, members of all political persuasions occupy leadership and committee positions, and we do not expect institutional factors to affect how a member votes; thus, the institutional variables are excluded from the vote model. We do expect institutional position to influence the timing of the announcement. To be an effective cue-giver, a leader needs to announce early. Because of disagreement in the Democratic leadership regarding NAFTA, we included variables that represent the *Republican Leadership* and *Democratic Leadership*. In addition, a *Committee Membership* variable was included in the timing model to test whether members of committees that took action on NAFTA also announced their decision early, since they could attempt to act as cue-givers based on their institutional position of committee expertise.[15]

Table 1 summarizes how the independent variables are expected to influence both direction and timing of the vote intention and gives summary statistics for all variables. (Appendix A provides detailed information about the coding of variables.) The table highlights the different expectations for the two models. It is the exception rather than the rule for the hypothesized effects to be the same. Both high and low values on union membership, Perot vote, and household income are expected to have positive effects in the timing model, whereas no independent variables are expected to have this nonlinear relationship in the vote model. The timing model also contains hypothesized interactions with the ideology variable. In the vote model, PAC contributions from corporations have a positive effect and those from labor, a negative effect; in the timing model, contributions are expected to have a positive effect regardless of the source. Institutional

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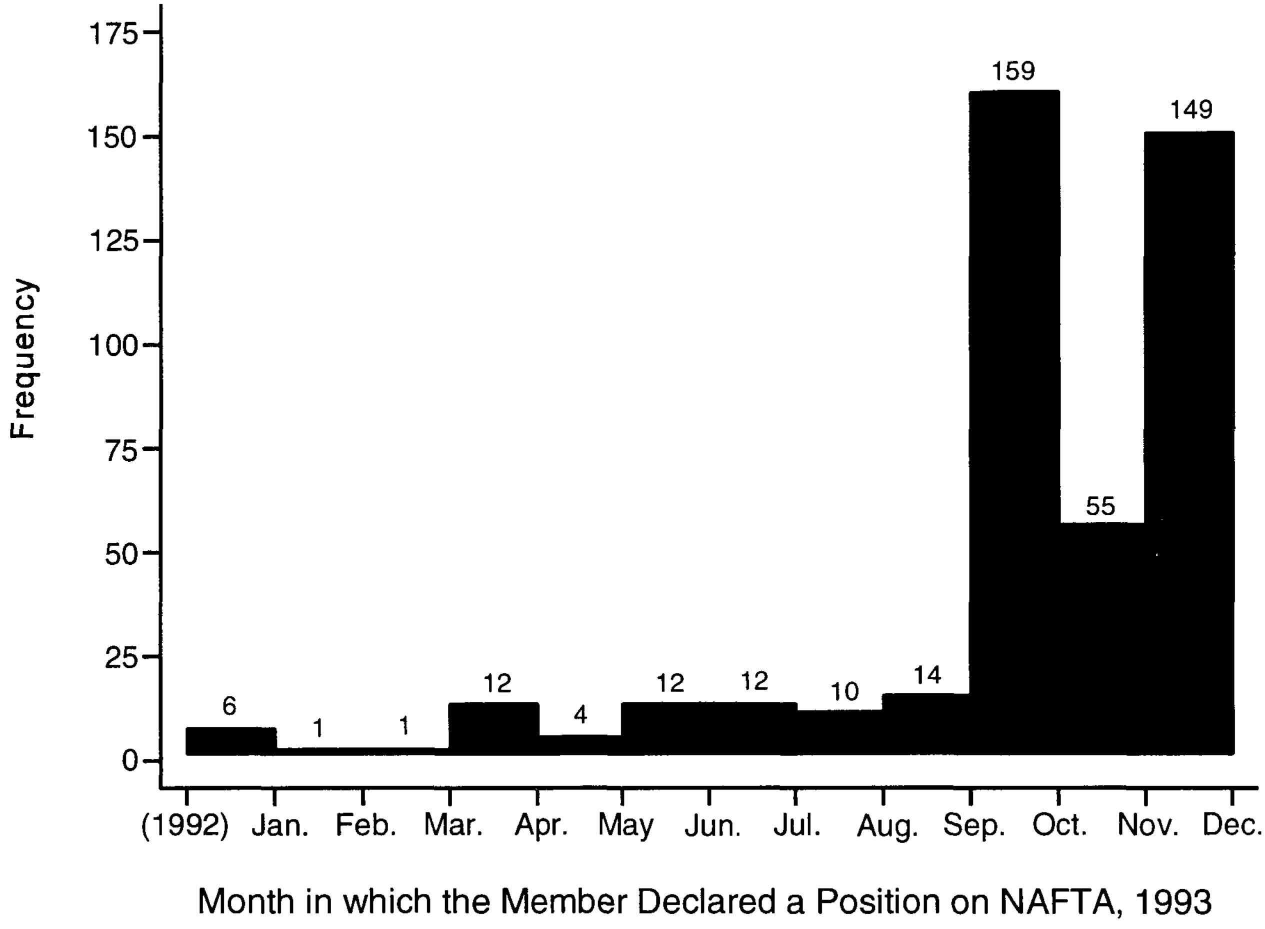
[15] For a more general model, committee action is important, but we recognize there is unlikely to be an effect because of fast-track procedures (see note 6). The NAFTA committee variable was coded 1 for all members on Ways and Means, Banking, and Energy and Commerce. All three committees took action on NAFTA. Alternative specifications were considered, including variables indicating only those members of a committee that made a recommendation (Ways and Means and Banking), members of a committee that had a recorded vote (Ways and Means), and members of any of the eight committees to which the legislation was referred. In no instance was the direction and statistical significance of the committee variable, the other variable effects, or the overall interpretation of the model affected.

FIGURE 1. Timing of Position Taking on NAFTA



factors, such as being part of the Republican leadership, illustrate the hypothesized effect of being a cue-giver to other members of Congress, which is examined in the dynamic timing model.

A DURATION MODEL OF THE TIMING OF POSITION ANNOUNCEMENTS

To test our model of the strategic timing of position taking on NAFTA, data were collected on the position taken and timing of the announcement for all 435 House members of the 103d Congress.[16] Figure 1 shows the distribution of the timing data aggregated by month.

The variable indicating the time at which a House member took a position on NAFTA is coded as the number of days after August 11, 1992, that the member first took either a "yes" or "no" position.[17] By selecting that date, we set the earliest recorded stand by a member to a duration of 1 and eliminated concerns with left censoring in the data.[18] Members who did not announce prior to the day of the vote are treated as announcing on that day and given a duration of 463.[19]

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[16] The announcement date is that on which any news source reported a member had indicated a willingness to support or oppose the legislation, that is, s/he was no longer undecided. Data on timing, to be adequate to the task, are collected at substantial cost. They were gathered through electronic searches on each member for the period prior to the vote using LEXIS/NEXIS. Not only did we search the usual sources, such as major national newspapers, weekly political journals, trade publications, local newspapers, and other periodicals, but also we searched congressional hearings, both on NAFTA itself and others in which the agreement was mentioned, and members' statements from the House floor. Finally, we incorporated data on positions as reported in six polls of members' offices conducted before the vote. We recorded the position and the date on which it was taken. Because of the infrequent votes by the Speaker of the House, a number of groups (including the Chamber of Commerce, which we used) did not calculate scores for Thomas Foley (D-WA), who is excluded here. In addition, there is no vote score for Vernon Ehlers (R-MI) because he replaced Paul Henry, who died in office. Our analyses thus are based on an *N* of 433.

[17] Representative Peter Visclosky (D-IN) announced his opposition on August 12, 1992.

[18] Censoring occurs when there is incomplete information about the duration of the variable in a particular state due to a limited observation period (Yamaguchi 1991, 3). In our case, observations go from the "state" of undecided to decided. If we started coding data from January 1, 1993, then any member who took a position before that would be left censored because we would not know how long s/he had been in the decided state.

[19] While some members did switch from their initial announced position, the number is not large. Thirty-five cast votes different from their announced position; of these, eleven switched to opposition and twenty-four to support. If we recode the timing and position of those who switched from the date of their initial announcement to the date

TABLE 2. Factors Influencing the Timing of Position Taking by Members of the House of Representatives on the North American Free Trade Agreement

Variable	Estimate	Standard Error	*p*-value	Percentage Change in the Hazard Rate
Constituency Factors				
Union Membership	3.21	1.19	0.01	3.3
Perot Vote, Percent	−4.91	4.27	0.25	−4.8
Perot Vote, Percent Squared	15.64	11.72	0.18	1.6
Mexican Border	1.84	0.32	0.00	527.9
Household Income	0.01	0.09	0.91	1.0
Interest Group Factors				
Corporate Contributions	−1.44	0.52	0.01	−1.4
Labor Contributions	1.09	0.50	0.03	1.1
Institutional Factors				
NAFTA Committee	0.04	0.11	0.74	3.7
Republican Leadership	0.56	0.26	0.03	74.5
Democratic Leadership	0.08	0.23	0.72	8.6
Individual factors[a]				
Interaction Effect of Ideology and Union Membership	−4.39	1.78	0.01	–
Interaction Effect of Ideology and Household Income	0.16	0.13	0.19	–

[a] See text for discussion of the interpretation of the interaction effects and their influence on the hazard rate.
Log-likelihood = −2233.96; Chi-square (12) = 53.95; $p < .001$; number of cases = 433

Duration analysis is ideally suited for estimating the effects of factors on the time until a position is taken.[20] We apply a proportional hazards model developed by Cox (1972) to the timing of House members' positions on NAFTA. The proportional hazards model allows one to estimate the effects of individual characteristics on a duration without having to assume a specific parametric form for the distribution of the time until a position is taken, thus imposing fewer restrictive assumptions than do parametric duration models. For a member of Congress with a vector of characteristics, **X**, the proportional hazards model assumes a hazard rate of the form

$$h(t|x) = h_0(t)e^{x'\beta}, \quad (1)$$

where $h_0(t)$ is an unspecified baseline hazard function.[21] The hazard rate may be interpreted as the risk that an event occurs at any point, given that an event has not yet occurred.

The independent variables are included in the proportional hazards model to assess their hypothesized effect on the timing of members' position announcements (see Table 1 for a summary). Perot's share of the total presidential vote in the district, as well as the square of this term, is included to capture the shape of the expected effect. Following Cronbach (1987), we "center" (subtract the mean from) the Perot support variable before squaring and the union membership and household income variables prior to their interaction with ideology. This procedure reduces the possibility of colinearity among the variables and their interactions as well as allows for a more natural interpretation of the estimated baseline hazard.

Results for the timing model are presented in Table 2. Overall fit of the model is good; we may reject the null hypothesis that the coefficients are jointly zero at the .001 level. The coefficients from a hazard rate model indicate the magnitude of the effect and whether a particular variable increases or decreases the hazard

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on which they declared their ultimate position, then the results are substantively the same as shown in tables 2 and 3. While position switching is itself an interesting issue, a different data set and methodological approach are required to examine why these members switched.

[20] See Beck (1996); Box-Steffensmeier and Jones (n.d.); Chung, Schmidt, and Witte (1991); and Kiefer (1988) for a review of duration models. In recent years these have been applied to a wide variety of political phenomena, including the duration of wars (Bennett and Stam 1996), regime change (Gasioroworski 1995), and the survival of political leaders (Bueno de Mesquita and Siverson 1995), and to the study of legislative behavior, for example, congressional committee assignments (Katz and Sala 1996), congressional elections (Box-Steffensmeier 1996), and the co-sponsorship of bills (Kessler and Krehbiel 1996).

[21] The model assumes that the hazard functions of all individuals differ only by a factor of proportionality. That is, each individual's hazard function follows exactly the same pattern over time, but the proportional hazards model puts no restriction on what this pattern can be; that is, it puts no restriction on the $h_0(t)$ curve, which determines the shape of the $h(t|X)$ curve. Thus, the coefficient vector β can be estimated without specifying $h_0(t)$. Also, $h_0(t)$ can itself be estimated nonparametrically and, thus, with substantial flexibility. The model does have some potential drawbacks. Because it uses only rank-order information on the timing of events, the original Cox model is inestimable in the presence of ties in the data, that is, more than a single observation occurring at the same time. A number of alternative estimation techniques have been suggested for such cases, and we adopt that proposed by Breslow (1974) and Peto (1972). Simulations using Monte Carlo data have shown that the use of the Peto approximation in the presence of ties in failure times may bias results downward (Farewell and Prentice 1980). Thus, our results should, if anything, underestimate the effect of the various covariates on the probability of a member announcing a position on NAFTA. We also ran the analysis as a parametric model, specifically a Weibull model, because of the concern about ties, and the results support the Cox model findings.

rate. An increase means that members with these characteristics are likely to announce position earlier. As is typically the case, the coefficient divided by the standard error indicates whether the effect is statistically significant, and the p-values give the precise probability values. In general, the signs and p-values support the hypotheses laid out in the previous section. Some constituency, interest group, institutional, and individual factors are all statistically significant.

The fourth column of Table 2 shows the percentage change in the hazard rate, which provides an insightful interpretation of the variables. For a dichotomous independent variable, the percentage change in the risk of experiencing the event in question is calculated as:

$$100[e^{(\beta_k*1)} - e^{(\beta_k*0)}]/e^{(\beta_k*0)}. \qquad (2)$$

The interpretation for a continuous independent variable is similar:

$$100(e^{[\beta_k*(x+z)]} - e^{(\beta_k*x)})/e^{(\beta_k*x)} \qquad (3)$$

for a z unit change in the independent variable, x.

Considering first the statistically significant dichotomous variables, we see that members whose districts border Mexico declared their position earlier than did those from other districts. Specifically, the hazard rate increases by 528%. This means that, at any time, the instantaneous probability of declaring a position if one's district borders Mexico is more than six times that of a comparable nonborder district. This confirms our expectation that early position taking was likely due to the high salience of NAFTA in border districts. Attentive constituents in these areas would be expected to raise the issue earlier than would constituents in other districts.

If the representative is part of the Republican leadership, there is a 74% increase in the hazard rate. The same effect is not seen for Democratic leaders, probably because they split on the issue. The other institutional factor, committee membership, was not statistically significant. As noted above, since NAFTA was considered under fast-track procedures, the typical committee role was changed, and therefore the lack of statistical significance is not surprising.

Our findings indicate that corporate contributions *decrease* the hazard rate by 1.4% for each unit increase (.01 or 1%), while labor contributions *increase* the hazard rate by 1.1% for each unit increase. An increase of one standard deviation in corporate contributions (around 10%) results in a 14% decrease in the hazard rate. In contrast, a similar deviation rise in labor contributions, which is 11%, corresponds to a 12.1% increase in the hazard rate. This means that there is a 12.1% increase in the propensity of a member to declare a position, given that s/he has not yet announced. These contrasting directional effects are likely due to the solidarity of labor opposition to NAFTA, whereas there was some division in corporate support. For example, Roger Milliken, chairman of a large textile corporation, warned against NAFTA: "A Congress dedicated to the survival of this nation's manufacturing base and the standard of living it provides our people should send the negotiators back to the drawing board" (PR Newswire Association 1992). The corporate dissension appears to have resulted in House members being cross-pressured and therefore delaying their position taking.[22]

Finally, we examine the results for the interaction of ideology and the two constituency variables. To interpret these effects, first consider the case of a liberal member, for whom the ideology dummy variable equals zero. In this case, the coefficient and standard error of the unionization variable reported in Table 2 represents the effect of district unionization on the timing of a member's announcement. That effect is positive; a 1% increase in unionization in a liberal's district increases the hazard rate by 3.3%. Contrast this with a conservative member, for whom the ideology dummy variable equals one. Now the unionization coefficient of 3.21 is added to the interaction coefficient of −4.39, yielding a unionization coefficient for conservative members of −1.18. This negative influence reflects cross-pressure from constituency and individual factors for conservatives, hence the negative effect on the hazard rate (and correspondingly later position announcement) for such members. The new standard error is calculated as:

$$\sqrt{var(\beta_d) + var(\beta_i) + 2cov(\beta_d,\beta_i)}, \qquad (4)$$

where β_d and β_i are the regression coefficients for the direct effect and the interaction term, respectively (Friedrich 1982, 810).[23] Here, the standard error for the interacted unionization coefficient is 1.37, meaning that although the coefficient is in the expected direction, it is not statistically significant. The interaction itself, however, is still substantively important and should not be removed from the model. Its inclusion allows us to see the differential effect of unionization for liberals and conservatives, that is, a meaningful difference in the influence of organized labor as between ideologically opposed members.

A similar examination of the interaction of household income and ideology reveals that, as district affluence increases, liberal members experience cross-pressures. While the effect for liberals is not statistically differentiable from zero, the coefficient for conservatives is statistically significant at the .05 level, indicating that the latter declare a position substantially earlier as district household income rises. Seen in this light, the lack of effect for liberal members implies that cross-pressures are at work, since a clear signal to take a position appears not to be received.

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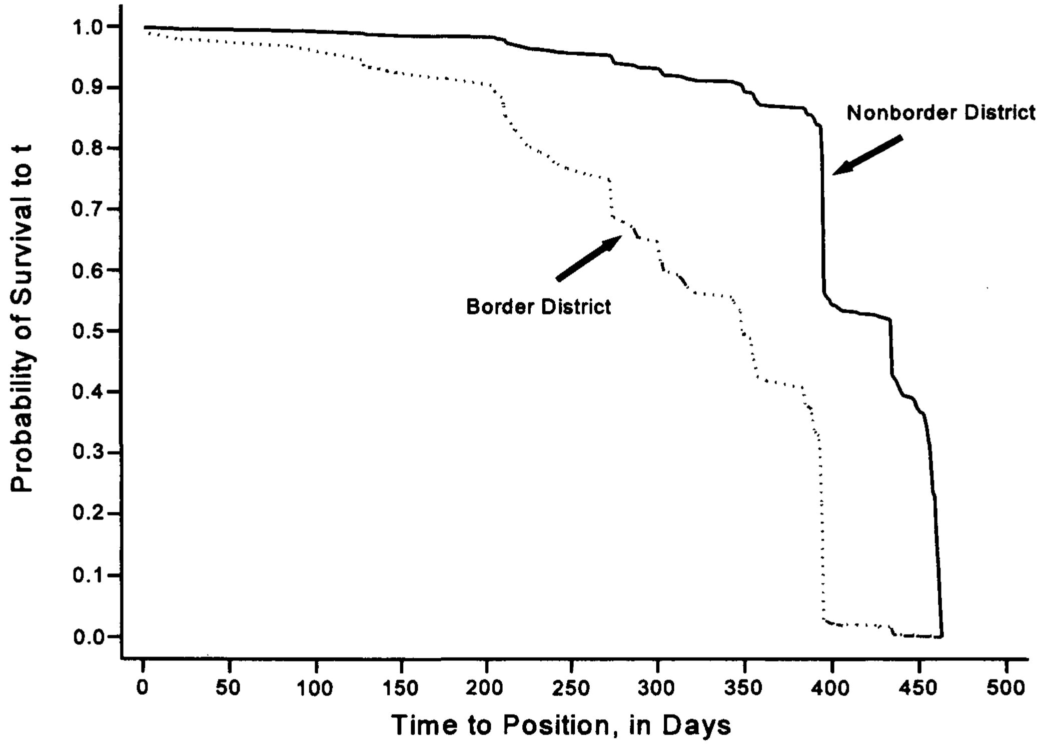
[22] The FEC uses six categories to categorize PACs: corporate, labor, trade/member/health, cooperative, corporate without capital stock, and nonconnected. There were 1,789 corporate PACs and 337 labor PACs in 1993 (Federal Election Commission 1995). It is possible that more refined measures of the interests of PACs, which could be determined using SIC codes, would clarify the sign on corporate contributions.

[23] The ideology variable does not enter the equation separately because Cox's proportional hazards model does not have an intercept; the constant term is absorbed into the baseline hazard rate. If the dummy were entered separately, then the model would contain an intercept when this variable is equal to 1.

FIGURE 2. Estimated Survival Probabilities: Mexican Border and Nonborder Districts



Another way to understand the effect of an independent variable on the timing of position taking is to look at comparative survival functions, which are generated by setting the independent variable of interest at various levels (Teachman and Hayward 1993). The survival function is simply one minus the cumulative distribution function of the duration. The relationship between the survival function and the hazard function is:

$$S(t) = \exp\left[-\int_0^t h(u)du \right]. \tag{5}$$

The survival function represents the cumulative probability that an individual survives, that is, does not take a position, to time t. In the proportional hazards framework used here, the survival function for an individual with covariates X_i can be written as

$$S_i(t) = S_0(t)^{\exp(\beta' X_i)}, \tag{6}$$

where $S_0(t)$ is the baseline survival function defined in equation 5. This clarifies how the set of covariates X_i enters the survival function. For example, comparisons of survival functions for a dichotomous indicator variable (say, D_i) are accomplished by setting the other covariates to the same values, for example, their means, for both values of D_i. The ratio of the survival functions for the two groups $D_i = 0$ and $D_i = 1$ can be written as:

$$\frac{S_i(t)^{\exp(\beta' X_i + \beta_D)}}{S_i(t)^{\exp(\beta' X_i)}} = S_i(t)^{\exp(\beta_D) - 1}. \tag{7}$$

In a similar fashion, we can calculate the influence of a change in a continuous independent variable on the survival function according to equation 7.

We illustrate this approach by contrasting survival functions for congressional districts bordering Mexico with those that do not. The large positive sign for this coefficient reflects higher hazard rates for border districts, which translate into reduced probabilities of survival at any given time. The effect of this variable is illustrated in Figure 2, which shows the substantial difference between the two types of districts. The mean difference in survival probabilities over the period examined is 0.35, a considerable difference in probability terms. This figure illustrates that the importance of the NAFTA issue in border districts was a substantial factor in causing members to announce their position early.

A similar analysis of comparative survival curves for the interaction of a constituency factor (unionization) and a membership factor (ideology) for conservatives and liberals, assuming a district in which percentage of unionization is two standard deviations (about 12%) greater than the national average, reveals that the mean difference between the survival curves is .12. This represents the average difference in the probability of survival between liberal and conservative members in such a district. Recall our hypothesis that, while higher unionization should work in favor of liberals reaching a decision, it will have the opposite effect for conservatives. The estimated survival probabilities for conservatives are higher than for liberals, reflecting the presence of cross-pressures. These result in conserva-

TABLE 3. Factors Influencing the Vote by the House of Representatives on the North American Free Trade Agreement

Variable	Estimate	Standard Error	p-value	Derivative at Means
Constant	0.27	0.33	0.81	
Constituency Factors				
Union Membership	−5.02	1.29	0.00	−1.99
Perot Vote, Percent	0.61	1.16	0.60	0.24
Mexican Border	0.34	0.43	0.43	0.13
Household Income	0.21	0.09	0.02	0.08
Interest Group Factors				
Corporate Contributions	1.94	0.72	0.01	0.77
Labor Contributions	−3.84	0.92	0.00	−1.53
Individual factors				
Ideology	0.08	0.31	0.81	0.03
Party Affiliation	−0.20	0.31	0.51	−0.08

Log-likelihood = −236.29; Chi-Square (8) = 125.16; $p < .001$; number of cases = 433

Actual and Predicted Outcomes

	Actual		
Predicted	No	Yes	Total
No	133	49	182
Yes	67	184	251
Total	200	233	433

Model predicted percentage = 73.2; null predicted percentage = 53.8; proportional reduction in error = 42%

tives delaying position taking and in correspondingly greater survival probabilities for such members. Similar effects, albeit in the opposite direction, are observed for liberals and conservatives with regard to district household income.

With the effects on the timing of announcements in mind, we turn to the model of the end of the process, the vote on NAFTA on November 17, 1993. The estimation of the dynamic timing model and the vote model allows contrasts to be drawn between the effects of the these two conceptually different processes.

A MODEL OF CONGRESSIONAL VOTING ON NAFTA

Results from our analysis of the House vote on NAFTA are presented in Table 3. The dependent variable is the member's vote on NAFTA, coded 1 for members who supported passage and 0 for those who opposed.[24] The overall fit of the probit model is good; many of the individual coefficients are statistically significant, and the log-likelihood ratio statistic indicates rejection of the hypothesis that the coefficients are jointly zero at the .001 level. The model predicts members' votes substantially better than the null, correctly predicts passage, and roughly approximates the actual margin of victory.

Examining the results for the individual variables in the vote model is more familiar and straightforward than in the dynamic timing model. A popular approach for interpretation in a nonlinear model is to compute the partial derivative of the expected (or predicted) value of the dependent variable with respect to the independent variable of interest.[25] Partial derivatives represent the instantaneous effect of a change in the independent variable in question on the dependent variable when the latter is at a preselected level (King 1989). We compute this statistic for each independent variable when all such variables are set at their mean.

Two of the constituency variables are shown to affect a member's vote, unionization and household income. Our measure of the district's unionization has a negative effect on the probability of the member voting for NAFTA. This variable has the strongest overall influence; both the coefficient itself and the partial derivative are substantially larger in absolute magnitude than the others. The median household income in a district has a statistically significant positive influence on the probability that the member would vote for the agreement, which is in keeping with our prior expectation that high-wage workers have more to gain from NAFTA than low-wage workers.

While much was made of Perot's outspoken stance against NAFTA, our results indicate that he had little, if any, direct influence on members' votes or the timing of their announcement.[26] Members from districts in which Perot received a high percentage of the 1992 presidential vote were slightly *more* likely to vote for the agreement, although the estimate is too imprecise for confidence in this relationship. One possible explanation is that, while Perot was a vocal opponent of NAFTA, his actual ability to influence Congress was limited by his diffuse support in members' districts.[27] This reflected the diverse anti-NAFTA coalition, which included such disparate figures as Patrick Buchanan and Ralph Nader as well as organizations ranging from the AFL-CIO to Greenpeace.

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[24] The final vote on NAFTA was 234 in favor and 200 opposed.

[25] The partial derivative of $E(Y)$ with respect to the independent variable of interest, evaluated with all independent variables set at their mean value, is a useful way to compare coefficients in a nonlinear model. For a probit model, it is computed as $\partial E(Y)/\partial(X_k) = \phi(\beta' X)\beta_k$. Because these values have the same scale factor, they allow a more intuitive interpretation of the relative values of the various coefficients (see Greene 1993). To facilitate interpretation, we calculate these derivatives for all continuous independent variables; for dichotomous regressors, we report the change in probability associated with a change of 0 to 1 in that variable.

A district's location along the Mexican border does not show a statistically significant effect on a representative's NAFTA vote, in sharp contrast to the border effects on timing. Most likely this difference is due to the potentially mixed effect of NAFTA on these communities. While many believed they would benefit from expanded trade opportunities with Mexico, others were concerned about potential job loss and environmental degradation. The salience of the issue forced members in these districts to take an early position, but the mixed effect meant that neither position consistently carried the day among border region representatives.

Interest group factors, such as contributions from corporate and labor PACs, affected a member's vote in the predicted directions. The relative magnitude of the two coefficients suggests that labor had a greater influence than business. This again may be due to a difference in the clarity of the signals sent. As noted previously, organized labor solidly opposed the agreement, but business opinion was mixed: Some favored the NAFTA as a means of opening new markets and spurring growth, while others doubted the ability of American industries, such as textiles and household glass, to compete with products made in Mexico (Thorbecke 1995). The partial derivative for labor contributions is nearly twice as large as that for corporate donations, indicating a notably stronger influence of this variable. Taken together, the results for the unionization and PAC variables indicate the force of labor opposition to the free-trade pact.

Neither individual factor is statistically significant. The positive direction for the ideology variable indicates that conservatives were more likely to vote in favor. A member's party affiliation had no discernible influence on his or her decision. This lack of partisan influence reflects the fact that passage was ultimately accomplished by a bipartisan coalition of Republicans and moderate Democrats, with a majority of Democrats (particularly the liberal wing) voting against the agreement.

A consideration of all the factors—constituent, interest group, institutional, and individual—shows that only the first two affected the vote. In contrast, all four sets of factors played a role in the timing of position announcements. This difference points to the potential for increasing our knowledge of the legislative process through methods that account for temporal as well as cross-sectional aspects of decision making.

CONCLUSION

This research extends our understanding of legislative behavior through a model of position taking and the timing of vote announcements. The dynamic timing component adds insight into the strategic behavior of legislators, institutional constraints on members of Congress, and strategies of interest group influence. In terms of congressional voting, and political decision making in general, a number of aspects of this study are significant.

Particularly interesting are our results on cross-pressures or conflict in the signals that members receive. This conflict is more precisely articulated when timing is considered and can only be captured in a dynamic framework. In contrast, models of congressional roll-call voting can only reflect the end of the process, the direction. Our findings are consistent with a signaling-based model of legislative position taking, in which members declare their stand on important issues earlier or later depending on the strength and clarity of signals received from constituents, interest groups, and policy leaders, while reconciling these signals with individual factors, such as ideology. This paper is a notable first step toward more detailed analyses of timing as a political phenomenon.

Timing also enhances understanding of the role of cue-givers in the legislature. Do members who hold what traditionally are considered cue-giving posts actually behave in ways that facilitate the exercise of influence over other members? Do certain institutional roles or responsibilities encourage members to act in certain ways? Does one's place in the institutional framework encourage a member to view the timeline of a bill differently from a member with no institutional responsibility for the legislation? In the case of NAFTA, Republican leaders declared their position significantly earlier than others, making them at least potential cue-givers for the rank-and-file, but this was not true for committee members.

Considering the effects of timing also allows us to discuss the strategic considerations of interest groups, who want the opportunity to make their case to and obtain issue agreement or perhaps advocacy from their district's representative. Timing provides information about the role of advocacy within the institution and its relationship to support from identifiable interests. While we found that interest groups did play a role in when members announced their decision, a number of questions about that influence remain, such as which members were contacted when, and how; which groups were more influential; and which groups had what effects on timing.

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[26] The congressional leadership and the media often emphasized the role of President Clinton in the battle over NAFTA, suggesting that his influence was a potent force in the debate. Uslaner (1995), for example, argues that presidential contact and previous support for the president had an effect. To examine this contention, we included a variable measuring Clinton's strength in congressional districts but found no evidence of systematic influence. This does not mean that Clinton had none, especially since there is anecdotal evidence that some deal-making occurred. For example, three members from Florida switched their declared position in the final week, crediting their change to an agreement about citrus (*CongressDaily* 1993, 2).

[27] Another possible explanation is the geographic pattern of the Perot vote, which was stronger in places such as Maine, Vermont, Montana, and Washington, where not much was at stake in the NAFTA game. Also, Perot did poorly in a number of southern states, where unionization is weak. We thank Martin Wattenberg for pointing this out.

Interesting contrasts were found in the effects of explanatory variables on the conceptually different aspects of timing and voting in the context of NAFTA. In the timing model, the functional form of some explanatory variables is different, interactions are added to capture the effects of cross-pressures, and more factors—constituent, interest group, institutional, and individual—affect the dynamic process.

In summary, we suggest that the study of strategic position taking and the timing of vote decisions is important for several reasons. It adds information about the context and sequence of decision making. The analysis more closely approximates members' strategic considerations and therefore provides a more complete picture of legislative voting behavior than studies which focus solely on outcomes. Finally, in contrast to most of the literature on legislative roll-call voting, which focuses exclusively on the end result, it examines more fully the *process*. Since even basic hypotheses about strategic timing issues in the legislative setting have not been articulated or systematically tested, this research is an important benchmark for future examinations of congressional voting behavior.

APPENDIX A: VARIABLES AND CODING

Dependent Variables

Vote. Representative's vote on H.R. 3450 (NAFTA implementing legislation), cast November 17, 1993. Coded 1 if voted in favor of passage, 0 otherwise.

Timing of Position. Duration variable indicating the number of days after August 11, 1992, until the representative took either a "yes" or a "no" position on NAFTA. "Undecided" and "leaning" positions were not included. This variable includes data from various polls conducted prior to the vote.

Independent Variables

Union Membership. Proportion of private-sector workers who belong to a union in the representative's district, 1991–92. Coded from *Current Population Survey.*

Mexican Border. Coded 1 if the district contains a land border with Mexico, 0 otherwise.

Household Income. Median household income in the district in thousands of dollars, as reported in the *Almanac of American Politics, 103rd Congress.*

Perot Vote. Proportion of the 1992 presidential vote in the district for H. Ross Perot, as reported in the *Almanac of American Politics, 103rd Congress.*

Corporate Contributions. Proportion of total contributions to the member's campaign committee from corporate political action committees.

Labor Contributions. Proportion of total contributions to the member's campaign committee from labor-related political action committees.

NAFTA Committee. Membership on one or more of the committees which acted on NAFTA implementing legislation. Coded 1 if representative was a member, 0 otherwise.

Republican Leadership. A position in the Republican Party of the House. Coded 1 if minority leader, conference chair, vice-chair, secretary, minority whip, chief deputy whip, deputy whip, and assistant deputy whip; 0 otherwise.

Democratic Leadership. A position in the Democratic Party of the House. Coded 1 if the Speaker, majority leader, caucus chair, vice-chair, majority whip, floor whip, ex-officio whip, chief deputy whip, and assistant deputy whip; 0 otherwise.

Party Affiliation. Coded 1 if Democrat, 0 if Republican. Minnesota Democratic Farmer/Labor Party coded as Democrat.

Ideology. Dummy variable based on 1993 Chamber of Commerce voting score. Coded 0 for a rating of ≤50; 1 otherwise.

APPENDIX B: UNION MEMBERSHIP DATA BY CONGRESSIONAL DISTRICT

A data set was created for union membership percentages in each of the 435 districts of the 103d Congress. Data came from the Area Union Membership Files compiled by Hirsch and MacPherson (1993), which are derived from the *Current Population Survey* (CPS). We mapped the data to congressional districts via the Consolidated Federal Funds Report (CFFR) Geographic Reference File for fiscal year 1992, available from the Bureau of the Census, U.S. Department of Commerce. Our data set includes information on private-sector labor union membership and total union membership (i.e., membership for all wage and salary workers, including private sector, federal, state and local government employees, and postal workers), expressed as a proportion of the total workers in the district.

The Area Union Membership File is an aggregation of the CPS data on individual union membership throughout the United States. It contains data for 247 Metropolitan Statistical Areas (MSAs) and 50 nonurban state areas. Individuals are counted as union members if they respond "yes" to the question: "On this job, is (the respondent) a member of a labor union or of an employee association similar to a union?" The CPS excludes self-employed individuals and military personnel. For each area, the number of union members is divided by the total number of wage and salary workers to obtain the percentage of unionized workers. The CFFR file contains information on 45,976 distinct geographic areas, comprising all 50 states plus territories. Included are codes for Bureau of the Census designations as well as state, county, and city government units. The number of congressional districts in the geographic area and their identity also are included.

We merged the Hirsch and MacPherson data on union membership percentages with CFFR data, so that each of the 45,976 subunits in the CFFR file was assigned a unionization percentage based on its location in one of the 297 MSA/state areas. This is an adequate representation of union membership in most instances, since the CPS data come from a Census-based probability sample of the entire United States. These data were then aggregated to the congressional district level by first making one copy of each observation for each congressional district in which it exists, so that every district contains a CFFR observation for all CFFR areas in its boundaries. We then accounted for a CFFR unit's contribution to the total union membership of the district by weighting a CFFR area's assigned membership percentage by its population, then summing over the weighted membership scores and dividing by the total (aggregated) population of the congressional district. In this way, the aggregation process accurately accounts for varying levels of population as well as union membership. CFFR areas that occupy more than one district were weighted and counted for each district in which they fall. Because there is no way to determine what percentage of a CFFR area falls in which of the two or more districts, and whether union membership varies between those respective parts, both districts were given a weighted measure of union membership based upon the CFFR area's entire population, rather than just the part located in each district. While this procedure introduces some level of bias due to varying population across CFFR areas, the problem is mitigated by the fact that 37,927 of the 45,976 CFFR units (82.5%) exist in only one congressional district, and only 24 CFFR areas are spread across 5 or more districts. To the extent that existing scholarship relies on *state* union membership totals when conducting district-level analyses, we feel our data are a significant improvement.

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Crime and Punishment: The Role of Norms in Crisis Bargaining

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In this work I test the proposition that normative standards of behavior can influence state actions in security-related conflicts. Specifically, I examine the construction of norms in the settlements of security-related disputes and the effects these settlements have on subsequent militarized interactions. I argue that dispute settlements alter subsequent crisis bargaining in two important ways. First, they act as normative referents that alter the interpretation of subsequent crisis bargaining behavior both by identifying a solution to the dispute which alleviates fears of demands for future concessions and by defining a set of acts which both sides consider illegitimate. Second, in combination with the response to their violation, dispute settlements inflict reputational costs on states that violate them. I test these arguments against a realist theory of crisis bargaining through an analysis of 122 reinitiated international crises between 1929 and 1979. I find strong support for the hypothesis that states can and do construct normative standards that guide their behavior in international crises, whereas realist theory receives only mixed support.

The realist paradigm, with its focus on international anarchy, national interests, and the central importance of relative power, has dominated the discussion of international relations—and security affairs in particular—over the past five decades (Carr 1946; Huth 1988; Mearsheimer 1983, 1994/1995; Morgenthau 1985; Waltz 1979). The inadequacy of realist explanations for the end of the cold war, however, has provoked some scholars to search for alternative paradigms as complements to or replacements for realism. In particular, recent scholarship has argued that the cold war collapsed as a result of changes in the beliefs and ideas of foreign policymakers in the Soviet Union and Eastern Europe rather than as a response to any change in the global distribution of power (Checkel 1993, Evangelista 1995, Koslowski and Kratochwil 1994, Mendelson 1993). This finding has led theorists to construct theories of international politics that are rooted in the influence of normative beliefs and ideas as well as material capabilities (Katzenstein 1996, Kocs 1994, Legro 1995, Paul 1995, Price 1995, Tannenwald 1997, Wendt 1992). Before complicating the elegant realist paradigm, however, we must investigate whether norms and ideas actually have a significant independent influence on state behavior. In particular, we must determine whether norms influence behavior in *military* conflicts, since these kinds of disputes are most central to a realist explanation of international relations.

The central aim of this work is to test the proposition that normative standards of behavior can influence state actions in security-related conflicts. I will examine the norms embodied in the settlement of security-related disputes in order to determine whether such settlements affect subsequent relations over the same disputed issue. Specifically, I will investigate whether states that violate a settlement during subsequent crises are more likely to capitulate when they are punished because their behavior conflicts with international norms regarding the maintenance of such settlements. I test this claim against a realist model of crisis bargaining through a quantitative analysis of 122 reinitiated international crises between 1929 and 1979. Briefly, my findings indicate that these settlements *do* shape state leaders' responses to crisis bargaining behavior in ways which cannot be explained by a purely realist framework. These results indicate that international security norms can and have helped to stabilize peace and cooperation, even between states that represent substantial security risks to each other. At the same time, however, traditionally realist variables—such as coercion and reputation for toughness—are central to my explanation of crisis outcomes. Thus, my argument represents an important step toward integrating normative approaches with the prevailing, yet incomplete, realist approach to crisis bargaining.

The remainder of the article is divided into six sections. In the first, I define the concepts central to my argument, review some of the persistent problems explored in the literature on international norms, discuss my solutions to them, and describe the data that these solutions direct me to examine. In the second section, I present two complementary arguments concerning the informational role of norms in shaping state bargaining behavior. The first argument emphasizes the role of norms as focal points for interpreting behavior, while the second focuses on international norms as reputational constraints. In addition, I derive several specific hypotheses that flow from these arguments. In the third section, I offer a set of realist hypotheses on crisis bargaining that act as controls on my analysis. In the fourth section, I present my research design, the population of cases, and the operationalization of the variables in the analysis. In the fifth section, I describe the results of my probit analysis and the marginal effects of the significant variables. Finally, I conclude by reviewing the implications of my findings

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for international relations theory and theories of crisis bargaining.

THEORETICAL AND EMPIRICAL ISSUES

My analysis of international norms focuses on what I call "legitimate" international agreements. To say that an agreement is perceived as legitimate implies that the contracting parties recognize both their obligation to adhere to its stipulations *and* that the other parties are within their rights to enforce compliance with the agreement "even, if necessary, by the use of force" (Diamond, Linz, and Lipset 1990, 9).[1] A state's accession to an international agreement is embodied in the signature of its official representative, but that alone does not imply the state has consented to the agreement. Consent is meaningful if the agreement is "responsive" to the wishes of the state. I define an agreement as "responsive" if the state has played a role in shaping its stipulations.

If an international agreement is legitimate, then the broad standards of behavior it defines may be termed norms. In a descriptive sense, a norm refers to a behavioral regularity, that is, the way an actor *usually* behaves, and it often refers to a pattern of behavior developed over an extended period. In a prescriptive sense, a norm refers to the way in which an actor *ought* to behave. This usage does not necessarily carry any connotations of frequent or usual behavior. In this work I refer to norms exclusively in their prescriptive sense; when I say that a state has violated a norm, I mean it has behaved in a way it previously recognized as illegitimate.[2]

Recent scholarship has identified three ways in which norms may affect state behavior (Duffield 1995; Tannenwald 1997). Adopting Tannenwald's terminology, I shall refer to these types of influence as constraining, enabling, and constitutive. Norms are constraining when they impose costs on violators. Thus, state leaders may *like* to behave in a particular manner but refrain because of the costs they would suffer. Norms are enabling when they serve as focal points, which I label normative referents, to help states coordinate their behavior. In this case, norms alter state behavior by helping them interpret the behavior of other states in an uncertain international environment. Finally, norms are constitutive when they actually shape state identity and preferences.

My claim about norms and crisis bargaining relies on both the constraining and enabling concept of norms. Thus, my argument is consistent with what has become known as the "institutionalist" paradigm but does not embrace more fundamental "constructivist" critiques of realism (Wendt 1992). I do not restrict myself to institutionalist premises because I reject constructivism out of hand; rather, I wish to show that even if we accept most realist assumptions about international relations, we still must revise realism to incorporate the role of norms and institutions.

By granting that state interests are exogenous, constraining and enabling arguments have the potential for providing strong evidence that the realist paradigm needs revision, even on its own terms. Unfortunately, scholars have had considerable difficulty in demonstrating that international norms and institutions are not purely the epiphenomenal result of common interests. The methodological issue here is one of spuriousness. Since norms, institutions, and cooperation are all caused by cooperative interests, norms and institutions may appear to cause cooperative behavior, when in fact they are only correlated with it because of their dependence on cooperative interests. My solution is to evaluate the effect of norms when state interests clearly conflict. Thus, rather than focus directly on norms as a source of cooperation, I demonstrate that the effect of norms can be observed without the persistent concerns about spuriousness by studying responses to the violation of norms.

Since I will examine the violations of security norms as embodied in legitimate dispute settlements, my analysis will focus on behavior in reinitiated international crises. In each case, a state initiated a dispute, which was resolved either with or without the conclusion of a legitimate dispute settlement, and then one of the states reinitiated the dispute over the same issue. The first state to threaten or take military action to alter the previous outcome is termed the challenger in the reinitiated dispute. The defender is the state against which the challenger's threat or action is directed. In each case, the defender was forced to use some kind of bargaining strategy in an attempt to persuade the challenger to yield. Finally, each reinitiated dispute is resolved in one of three ways: (1) The challenger remains intransigent and forces concessions on the defender, (2) the challenger accepts a compromise, or (3) the challenger abandons its bid to overturn the status quo. I will focus on the challenger's response to the defender's bargaining strategy. That is, I hope to predict whether the defender will be able to persuade the challenger to abandon its revision of the status quo.

A THEORY OF NORMS AND CRISIS BARGAINING

My specific contention is that legitimate dispute settlements make it easier for defenders to protect the mutually agreed upon status quo for two complementary reasons. First, dispute settlements alter the challenger's interpretation of the defender's bargaining behavior in subsequent disputes by making coercive strategies appear more legitimate and less threatening. Second, dispute settlements, in combination with the defender's bargaining strategy, create substantial reputational costs for violators.

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[1] My definition of legitimacy is drawn from this work and more generally from the literature on the legitimacy of democratic governments. One source in which I relied particularly is Herzog 1989.

[2] Descriptive norms may work in similar fashion by providing focal points for expectations. I restrict my attention to prescriptive norms because arguments and research designs for studying the two types of norms differ.

Dispute Settlements as Normative Referents

I hypothesize that dispute settlements will act as normative referents which shape the interpretation of bargaining strategies. In particular, settlements provide the contracting parties with two important pieces of information that alter the context of subsequent crises. First, these agreements communicate to each state that its opponent is not fundamentally hostile. That is, they provide a focal point for resolving the crisis, which both states recognize that their opponent prefers to an escalation of the conflict. The anarchic nature of international politics makes it difficult to determine the true preferences and intentions of other states (Mearsheimer 1994/1995). This pervasive uncertainty often generates strong fears on the part of national decision makers, especially with regard to security affairs. By their very nature, security interests touch on core values of both national leaders and their constituents, such as survival and national sovereignty. The sensitivity of security interests makes state leaders extremely wary of exploitation in such matters. Consequently, they are reluctant to make any concessions in international crises for fear that these will be interpreted as a sign of weakness, which may cause the opposing state to escalate its demands. Such vigilance may successfully shield states from potential aggressors, but it often unnecessarily provokes hostile actions toward states with no aggressive intent (Jervis 1978). I hypothesize that dispute settlements can break this spiral of conflict by acting as a focal point which persuades each state that its opponent is not unalterably aggressive. In particular, if the defending state uses a highly coercive bargaining strategy following the violation of the settlement, then the challenger can remain confident that the defender will not demand concessions which move beyond the previous settlement. This knowledge of the opponent's intentions alters the interpretation of subsequent crisis bargaining behavior. Threatening and potentially aggressive behavior will no longer necessarily be categorized as a sign of aggressive intent. While outright aggression remains a possible interpretation for such actions, the fact that the opposing state has demonstrated its willingness to compromise will encourage state leaders to search for other possible interpretations.

Second, I argue that dispute settlements introduce the concept of legitimacy into a relationship by defining for each of the contracting parties a set of proscribed acts. The anarchic nature of international politics has created a context in which notions of "right" and "wrong" are not useful categories for describing state behavior. Realist theory, for example, holds that states defend their interests and that labeling such actions as "good" or "bad" is beside the point (Walzer 1977; Carr 1946 is an exception). By consenting to a legitimate dispute settlement that proscribes a particular set of acts, however, the contracting parties create a normative dimension to their relations which gives them new categories for interpreting behavior. Because each party recognizes this new normative dimension, the conclusion of a settlement enables leaders to understand both their own actions and those of the opposing state as "legitimate" or "illegitimate" with regard to the issues covered by the settlement in a way that was not possible before its conclusion.

Dispute Settlements as Reputational Constraints

In addition to acting as normative referents, dispute settlements inflict reputational costs on violators. Jervis (1970) has outlined a number of costs states suffer by deceiving other states, most of which fall under the label of audience costs. My argument focuses on the costs that international audiences (i.e., other governments) may impose on state leaders if they do not keep their commitments. In particular, such deception can have a devastating effect on a state's bargaining reputation, which is a crucial asset because it allows states to achieve many objectives that otherwise would be impossible. For example, deterrence theory emphasizes the importance of a reputation for toughness in order to prevent international crises from escalating to the point of war. Despite the centrality of a reputation for toughness in realist theories of crisis bargaining, these theories often ignore the importance of other kinds of reputations. Cooperation theory, for example, focuses on the role of a reputation for trustworthiness in resolving international crises (Axelrod 1984, Oye 1985). Just as the reputation for toughness is important in deterring an attack or escalation of force, a reputation for upholding one's word is critical if states are to convince others to cooperate with them (Larson 1992).

The notion that international agreements are powerful because states invest their reputation in them is not new. This argument implies that a state should be wary of violating a dispute settlement because of the damage to its reputation for trustworthiness, but it has little to say about the central focus of this work: the resolution of disputes *after* an agreement has been violated. By adding a corollary to this argument, however, I generate some hypotheses about how reputational concerns affect bargaining after a violation occurs. In particular, I hypothesize that the reputational costs of violating a settlement will depend significantly on the response of the injured party. For example, if the defending state makes no response, then the challenger will suffer relatively fewer reputational costs for at least three reasons. First, the lack of a vigorous response will limit the amount of international attention given to the violation. The event is more likely to be handled at a lower level of bureaucratic authority, and prominent decision makers in other states are not likely to spend much time on the issue. Second, if the injured state does not object vehemently, then others may view this as tacit consent to the unilateral alteration of the settlement. The damage to the trespassing state's reputation is limited because its actions are less likely to be viewed as illegitimate. Third, in the absence of an attempt to enforce the previous settlement, it is difficult for others to infer how seriously the challenger takes its international commitments. Rather than regard the trespasser as a renegade who cannot be trusted to uphold any agreement, for instance, states may make the more limited inference that the trespasser will cheat if it is *allowed* to do so but will be sufficiently trustworthy if another state is willing to enforce the terms of a common understanding. According to this same logic, if the challenger continues to violate a settlement even after the injured state has tried vigorously to enforce the accord, then there will be much larger reputational costs for the trespasser. The strong response of the injured state is likely to draw greater international attention; no state could construe the trespasser's actions as a consensual revision of the previous agreement; and it would be clear to all that the trespasser cannot be held to its obligations even by the threat or use of force.

The Realist Hypothesis on Crisis Bargaining

In presenting specific hypotheses, I begin by considering how challengers will respond to various bargaining strategies in the *absence* of a previous dispute settlement. After establishing the realist null hypothesis against which my argument about norms should be judged, I turn my attention to how normative referents and reputational costs should alter the challenger's response to the defender's bargaining strategy.

Previous work on crisis bargaining identifies three strategies, ranging from least to most coercive (Huth 1988, Leng and Wheeler 1979). A strategy of appeasement, which exerts little or no coercive pressure, offers concessions in order to accommodate the challenger's demands. A firm-but-flexible strategy applies a moderate amount of coercive pressure by resisting the challenger's demands and threatening retaliation, while at the same time offering alternative compromise solutions. Finally, a bullying strategy, the most highly coercive of the approaches, makes counterthreats and demands unconditional compliance.

States use these bargaining strategies in international crises to convey their resolve in defending their interests balanced with their desire to avoid war. This work focuses on the effect of the defender's bargaining strategy on the challenger's response in a crisis. Thus, I now describe the challenger's expected responses to each of the defender's possible bargaining strategies. Following the challenger's initiation of the crisis, appeasement conveys an appearance of weakness on the part of the defender because it indicates that the defender's desire to avoid war outweighs its resolve to protect its security interests. Under these circumstances, the challenger is likely to view the crisis as an opportunity to improve its position. Thus, appeasement is likely to make the challenger more insistent on overturning the status quo.

A moderately coercive response, however, represented by the firm-but-flexible strategy, is more likely to persuade the challenger to back down. In his discussion of extended deterrence crises, Huth (1988) argues that the firm-but-flexible strategy conveys a dual message to the challenger of both resolve and restraint.

> Opposition to the demands of the potential attacker demonstrates determination to support the protégé, whereas conditional offers of compromise signal to the potential attacker the possibility of reaching an agreement which protects its political position and bargaining reputation. If the potential attacker has decided to back down from its initial threat, the defender's offer of conditional compromise may provide the means to de-escalate (Huth 1988, 52–3).

The moderately coercive firm-but-flexible strategy conveys strength without triggering fears of aggression by the defender.

Finally, bullying behavior should be less successful than a firm-but-flexible stance because it emphasizes the defender's resolve to stand firm without giving the challenger an acceptable way to extricate itself from the crisis. In response to strongly coercive tactics, the challenger will fear making concessions because they may encourage additional demands by the defender. The challenger fears developing a reputation for weakness, which may result in future aggression. It is unwilling to make concessions, even to return to the status quo ante.

HYPOTHESIS 1: *In the absence of a previous dispute settlement to guide bargaining behavior, the relationship between the coerciveness of the defender's bargaining strategy and the challenger's insistence on overturning the status quo will be described by a concave curvilinear function. That is, appeasement will make the challenger highly insistent, a firm-but-flexible strategy will make the challenger less insistent, and bullying strategies will make the challenger highly insistent on overturning the status quo.*

This hypothesis represents the conventional wisdom on international crisis bargaining behavior and emerges from existing realist scholarship.[3] I do not wish to assert that the logic behind this argument is wrong, only that it is incomplete. This conventional view ignores both the role of norms as referents and the reputational costs of violating them. For both these reasons, the bargaining strategies taken in reaction to the violation of a dispute settlement will elicit a different response from the challenger in a crisis.

Hypotheses on Norms and Crisis Bargaining

While dispute settlements can influence crisis bargaining in two different ways, by serving as normative referents and reputational constraints, both ways make identical predictions about the nature of crisis bargaining following the violation of a settlement. First, consider the influence of normative referents. In the absence of a previous settlement, I have argued that bullying tactics by the defender will cause the challenger to fear additional demands for concessions and to increase its insistence on overturning the status quo. If the challenger is in violation of a dispute settlement, however, then highly coercive tactics should not result in intransigence. The previous settlement is a mutually recognized focal point for the solution of the current crisis. The challenger can retreat to the terms of the settlement without risk of signaling to the defender that it will make even further concessions on the issue at hand. Moreover, the defender's consent to the previous settlement demonstrates to the challenger that it is not inherently aggressive. Thus, the challenger will reject that notion and will search for alternative interpretations of the defender's bullying. In doing so, because of the normative dimension created by the dispute settlement, the challenger will reframe its own violation as an "illegitimate act." In other words, the defender's bullying becomes a "legitimate" defense of the settlement and a punishment rather than an act of aggression. Thus, if the defender uses a highly coercive strategy following the violation of a settlement, then the challenger will retain its belief that the defender is not aggressive and will have learned that the defender remains vigilant. Consequently, it will comply with the defender's demands and yield in its challenge to the settlement.

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[3] This hypothesized relationship follows from the works of Huth and Leng, among others, concerning crisis bargaining. Huth (1988) derives his expectation for this relationship from deterrence theory, whereas Leng attempts to distinguish this argument from realism and deterrence theory (Leng 1993, Leng and Wheeler 1979). Upon closer examination, however, Leng's version of realism includes some assumptions about the nature of the opposing state that are not central to realism or deterrence theory (Leng 1993, 6–10). Specifically, Leng appears to conflate what Jervis (1978) labeled the "deterrence model" with deterrence theory. Thus, Leng's expectations about crisis bargaining are consistent with most realist approaches. For a critique of the conflation of deterrence theory with the deterrence model, see Huth and Russett 1990.

Conciliatory responses to the violation of a settlement will have precisely the opposite effect on the challenger's response. As noted above, appeasement is likely to be viewed as a sign of weakness if no previous settlement has been violated. In the case of a violation, however, the challenger recognizes it is committing an act that both it and the defender define as illegitimate. By appeasing a violation, the defender is abandoning the mutually recognized focal point for the resolution of the crisis. Thus, the challenger will view an appeasing response to a violation as an even *stronger* sign of the defender's weakness than would have been the case in the absence of a settlement. Given the opportunity for cheap gains, the challenger will remain stubborn in its bid to revise the status quo.

Finally, as was true without a dispute settlement, a firm-but-flexible strategy following a violation should be effective in persuading the challenger to yield. Such moderation conveys vigilance in enforcing the norm without triggering fears of additional demands by the defender.

The reputational effects of dispute settlements should have an identical influence on subsequent crisis bargaining. First, bullying by the defender in response to a violation will inflict substantial reputational costs on the challenger if it remains intransigent. Other states will view it as extremely untrustworthy, since it has demonstrated that it cannot be held to its commitments even by the threat or use of force. In contrast, conceding to the defender gives the challenger an opportunity to rehabilitate its reputation for keeping its word. While some reputational damage may be inevitable, by reestablishing compliance with the norm, the challenger demonstrates that it is not unalterably aggressive and can be held to its word by a vigilant defender. In addition, by offering concessions after committing a violation, the challenger does less damage to its reputation for toughness than may be the case in other contexts. Because the previous dispute settlement is a focal point for resolving the crisis, it is easier for the challenger to frame its concession as an act of compliance with the specific principles of the agreement rather than as a general signal that it lacks resolve.

If the defender responds with appeasement following a violation, however, then we should expect the challenger to insist on overturning the status quo. The violation will attract little international attention, and the states which do notice will either infer tacit consent by the defender or will make a very limited inference about the challenger's trustworthiness. In choosing to violate the settlement, the challenger knew that it risked some damage to its reputation, even if the defender were to respond with appeasement. Thus, in the absence of reputational damage beyond these expected costs, the challenger is likely to remain intransigent. Moreover, by agreeing to the previous settlement, the defender placed its own reputation for toughness on the line by explicitly committing itself to the status quo. Once again, I hypothesize that opposing the violation of a settlement should signal the defender's weakness even more strongly than if no settlement existed. This weakness should encourage the challenger to be even more insistent on overturning the status quo.

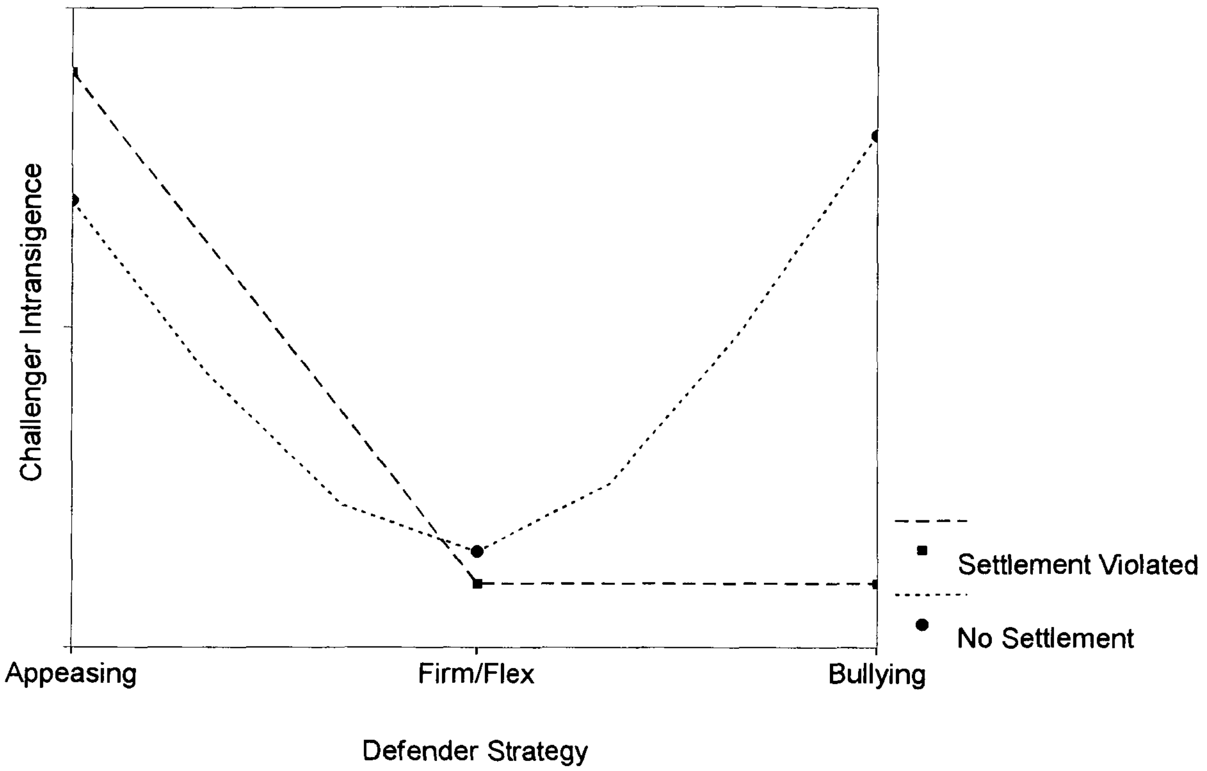
Finally, as was the case without a dispute settlement, a firm-but-flexible strategy should persuade the challenger to yield. This response by the defender will attract international attention to the violation, and continued intransigence will do severe damage to the challenger's reputation for trustworthiness, indicating that the violator cannot be held to its word even by force. If the challenger chooses to concede, then the presence of the settlement and the face-saving compromises incorporated in the defender's firm-but-flexible strategy will limit the damage to the challenger's reputation for toughness.

HYPOTHESIS 2: *When a challenger violates a dispute settlement and the defender responds with appeasement, the challenger's insistence will be higher than it would if there were no previous settlement.*

HYPOTHESIS 3: *When a challenger violates a dispute settlement and the defender responds with a bullying strategy, the challenger's insistence will be lower than it would if there were no previous settlement.*

HYPOTHESIS 4: *When a challenger violates a dispute settlement and the defender responds with a firm-but-flexible strategy, the challenger's insistence will be at least as low as it would if there were no previous settlement.*

FIGURE 1. Predicted Relationships between Challenger Intransigence and Defender Strategy



The competing predictions of the conventional model and my norm-based argument concerning the relationship between the defender's bargaining strategy and the challenger's insistence on overturning the status quo are displayed in Figure 1. The circle and dotted line represent the conventional wisdom about crisis bargaining. My model suggests that this prediction will be correct only for cases in which no previous dispute settlement has been violated. If the challenger has violated a settlement, I predict that the relationship between the defender's strategy and the challenger's response will shift to the one described by the square and dashed line. In this case very weak strategies will make the challenger even more insistent on overturning the status quo. In both models the influence of moderate coercion is about the same, but in my framework highly coercive strategies are effective in persuading the challenger to return to the status quo.

THE REALIST ALTERNATIVE: RATIONAL COERCION THEORY

In testing my argument about the influence of norms, it is important to control for the prominent realist alternatives to this approach. Deterrence theory and coercion theory are the most clearly established competitors to my argument (Ellsberg 1968; Schelling 1960, 1966). In addition, since my hypotheses concern behavior in reinitiated crises, it is important to consider realist arguments that focus on the evolution of conflict over time, such as those which emerge from the literature on enduring rivalries.[4] Drawing on these three literatures, I have developed a list of variables central to any realist explanation of crisis outcomes and include them as controls on my analysis: (1) *The Defender's Bargaining Strategy*, (2) the *Balance of Military Capabilities*, (3) the *Defender's Bargaining Strategy and the Balance of Military Capabilities*, (4) the *Possession of Nuclear Weapons*, (5) the *Interests at Stake in the Dispute*, (6) *Concurrent Involvement in Other Disputes*, (7) *Behavior in Previous Disputes* between the same adversaries, (8) the *History of Previous Conflicts*, and (9) the *Change in the Balance of Military Capabilities* since the most recent dispute between the adversaries.

The Defender's Bargaining Strategy

The realist predictions about bargaining behavior, described in hypothesis 1, are identical to those of my normative model *in the absence of a dispute settlement*. The crucial difference between these two approaches is that coercion theory does not incorporate the possible construction of norms or the way they may alter crisis bargaining. In fact, it explicitly rejects the notion that the standards of legitimacy constructed by dispute settlements will have any effect on crisis behavior (Mearsheimer 1994/1995).

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[4] Despite the obvious importance of rivalries in international politics, this literature is still in a relatively early stage of development. Moreover, as Hensel (1994) and Goertz and Diehl (1992) note, much of the work on enduring rivalries uses this concept as a case selection device to test arguments not directly related to the issue of rivalry (Huth and Russett 1993; Huth, Bennett, and Gelpi 1992). Thus, my hypotheses are restricted to some of the very basic ideas underpinning the notion of rivalries, such as a concern with reputation for toughness and the possibility of a positive feedback cycle that drives conflict escalation.

The Balance of Military Capabilities

Perhaps the most basic variable in any realist approach to international conflict is the balance of military capabilities. States with an advantage in these capabilities relative to their adversaries face lower costs in the event of military conflict than do the disadvantaged states. Consequently, dominant states may exploit their position by coercing others into making concessions or by simply imposing their will through victory on the battlefield. Weaker states may make concessions in order to appease powerful adversaries and to avoid the costs of military conflict.

The Defender's Bargaining Strategy and the Balance of Military Capabilities

Although a disadvantage in relative military capabilities will have some influence on a state's behavior, this effect may not be substantial if the more powerful state does not demonstrate the willingness to use its capabilities. Engaging in military conflict always entails some costs for both sides, and even a very weak state need not make concessions unless a powerful state has indicated its willingness to use force. This suggests that military capacity will be more effective in extracting concessions when it is complemented by highly coercive bargaining tactics.

Possession of Nuclear Weapons

Although nuclear arms are part of a state's military capability, their fundamentally different nature has caused most students of international conflict to treat them separately from conventional armaments (Glaser 1990, Powell 1990, Zagare 1987). One central argument of this literature relates to first-strike capacity, or the ability to destroy or disable all the opponent's deliverable nuclear warheads by launching first. The defender's possession of a first-strike capability has obvious and damaging implications for the challenger, since it sharply increases the challenger's costs in the event of military conflict and lowers the likelihood that the challenger will prevail on the battlefield. Both these factors will lead the challenger to make concessions in order to avoid an escalation.

Interests at Stake in the Dispute

A number of scholars have drawn attention to the importance of the interests at stake in a particular dispute (Betts 1987, George and Smoke 1974, Jervis 1984). If the defender has much at stake, then it will be willing to suffer considerable costs in order to achieve its goals, and acquiescing to the challenger's revision of the status quo, is relatively less attractive. As a result, the challenger will be cautious about a violation which concerns the defender's core values, since the defender may be willing to suffer substantial costs rather than concede. When disputes do arise over such issues, the challenger is more likely to yield.

Concurrent Involvement in Other Disputes

The resources a defender may direct toward a confrontation with a challenger depend, in part, on the defender's involvement in disputes with third parties (Huth, Bennett, and Gelpi 1992; Huth, Gelpi, and Bennett 1993; Huth and Russett 1993). A state occupied with war on one of its borders, for example, is less likely to be willing or able to threaten force against another neighbor. If the defender has committed diplomatic, political, and military resources to another dispute, then it is less likely to prevail against the challenger.

Behavior in Previous Disputes

Much of the work on deterrence and coercion—in theory and practice—is directly concerned with the importance of bargaining reputation.[5] A state which capitulates during a crisis knows that its bargaining reputation is damaged. Backing down in a previous crisis will be viewed by others as a reliable signal that a state is weakly resolved (Nalebuff 1991, Powell 1990, Schelling 1966). Since the challenger should perceive the previous capitulation of the defender as a sign of general weakness, it will tend to be intransigent in subsequent encounters with the defender, confident of its ability to prevail.

History of Previous Conflict

The literature on enduring rivalries demonstrates that relatively few dyads account for a substantial share of all international military conflicts (Gochman and Maoz 1984, Goertz and Diehl 1992). This has led to speculation about whether the dynamics of repeated conflict create a cycle of increasingly frequent and violent disputes (Hensel 1994). In presenting the "modified realist" model of territorial rivalries, for example, Huth (1996) argues that rivalries become embedded in domestic political debates within the disputing states. As a conflict persists, it becomes increasingly difficult for state leaders to make any kind of concessions to their adversaries because of the domestic repercussions. Taking a slightly different approach to the same question, Leng (1983) maintains that states tend to learn "realist" lessons about the dynamics of international conflict as they engage in repeated disputes. These lessons cause states to rely on increasingly coercive bargaining strategies as crises recur. Both arguments suggest that the challenger is more likely to be intransigent if there is an extensive history of conflict with the defender.

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[5] The literature on this point is far too extensive to cite exhaustively, but perhaps the most influential work is Schelling 1966.

Change in the Balance of Military Capabilities

According to realist theory, the status quo established by the outcome of a crisis reflects the relative power of the two states at that time. This suggests that if the balance of military capabilities has changed between a previous crisis and a current one, then the newly more powerful state should be able to improve its position relative to the status quo.[6] If the balance shifts in favor of the challenger, then it should be less likely to yield in its insistence on changing the status quo to reflect its increased capabilities. If the defender becomes more powerful, then the challenger should be more likely to comply with the defender's demands.

RESEARCH DESIGN AND MEASUREMENT

The cases for my analysis are drawn from the crises identified by Wilkenfeld, Brecher, and Moser (1988) in the International Crisis Behavior (ICB) Project between 1929 and 1979. Using the ICB data, I compare crises that involve violation of a previous settlement with crises that also involve reinitiation of disputes over the same issue but no previous settlement. It is important to compare crises which involve violations of settlements to other reinitiated crises, and to omit all other crises, to avoid conflating the influence of international norms with the simple salience of the status quo. One might expect, for example, that once an issue has been disputed in an international crisis, the status quo established by that crisis would become a salient point for settling subsequent disputes, regardless of whether both parties consented to that status quo. By comparing violations of settlements to other reinitiations, I can separate the normative content of consensual dispute settlements from the simple salience effect of the previous status quo. Thus, the final population of cases in my analysis is 122 international crises between 1929 and 1979 that reinitiated previous disputes during the same period. The cases are listed in the Appendix.

For each case, in addition to the primary challenger and defender, a number of other states may play supporting roles. These are of interest because they may shift the balance of military capabilities between the challenger and defender. Therefore, I include a state as a supporting actor only if it takes or threatens some direct military action on behalf of one of the disputants. Using these rules, the identification of challengers, defenders, and supporting parties is relatively straightforward. An exception is the direct engagement of a Great Power's military forces, in which case the outcome of the crisis is affected much more by the major power's behavior than by the actions of any minor power. In these instances, since my statistical model includes the bargaining behavior of the defender but not that of its supporting actors, the Great Power is coded as the defender, and the minor power is coded as a supporting actor.[7]

Since crisis participants are, by definition, self-selected and do not represent a random sample of international interactions, the analysis of any crisis behavior raises the issue of selection effects. These can only become more prominent in reinitiated crises, which involve *two* stages of self-selection. This nonrandom sample selection process and its effect on the estimated coefficients in my model do not imply that the results are "wrong" or that the coefficients are "biased." The coefficients are correct for the population of cases for which they have been estimated: *reinitiated crises*. The error researchers often make, however, is to generalize their results from nonrandomly selected samples to other populations (Achen 1986). Although the generalizability of my findings is limited to crisis situations, the advantage is a more difficult test for my normative argument, since the challenger in a reinitiated crisis should be especially motivated to overturn the status quo. I will return to the issue of selection effects in discussing the results.

OPERATIONALIZATION OF VARIABLES

The Challenger's Response

The *Challenger's Response* is the dependent variable in this analysis. The challenger's final response to the defender refers to the outcome of the crisis. The coding procedure for this variable draws on Leng and Wheeler (1979) and uses a three-point ordinal scale to reflect increasing insistence by the challenger on altering the status quo. The three categories are compliance, compromise, and intransigence. *Compliance* is coded 1 and involves two kinds of responses by the challenger. First, of course, the challenger may agree to all the defender's demands. Second, the challenger may attempt to placate the defender by giving in to some demands and/or offering alternative concessions without making any threatening actions. *Compromise* (coded 2) refers to a challenger who concludes the crisis by offering some concessions while remaining firm about other aspects of its challenge. There are two substantial distinctions between placation, which is coded as compliance, and compromise. In compromise not only does the challenger offer less than demanded but also the challenger's refusal to comply completely with the defender's demands is backed up by the threat of force. In placation the concessions offered are substantively comparable to those demanded by the defender. *Intransigence* (coded 3) includes two possible responses by the challenger: ignore the defender's demands and refuse to alter behavior, or defy the defender by escalating demands or military activities. In coding the challenger responses, I relied primarily on the sources and case summaries provided by the ICB data set as well as *Keesing's Contemporary Archives*.

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[6] I would like to thank Chris Achen and Stephen Krasner for bringing this argument to my attention.

[7] A related coding issue is the fact that many of the agreements and crises are multilateral. If the outcome of the interaction among the various actors was determined independently, then I separated the multilateral crisis into several dyads and included them separately in the analysis. If the outcome was codetermined, then the crisis remained as a single case, and I identified the primary challenger and defender based on their centrality in defining the issues at stake and the outcome of the dispute.

The Defender's Bargaining Strategy

The Defender's Bargaining Strategy is coded according to the framework used in previous research on crisis bargaining (Huth 1988). The three categories—appeasing, firm-but-flexible, and bullying—reflect increasing coerciveness in the defender's bargaining strategy. In general, bullying strategies respond to any challenger behavior other than compliance by continuing or escalating threatening actions. Firm-but-flexible strategies respond to compliance or placation by repeating the defender's position or offering further inducements but escalate the level of hostility in response to being defied or ignored by the challenger. Huth (1988, 65) writes that the "critical difference between the bullying and firm-but-flexible strategies is that in the latter case the defender does more than react negatively to the demands of the potential attacker: it also initiates proposals for compromise." Finally, appeasing strategies respond to anything but compliance by offering further inducements to the challenger.

In order to code this variable, each crisis was broken down into a series of actions and counteractions. Relying on the procedures used by Huth and developed by Leng (1989, 1993; Leng and Wheeler 1979) in order to categorize each coercive strategy, I examined each response of the defender to the bargaining position of its adversary.[8]

Leng actually begins with five categories, adding "trial and error" and "stonewalling" to those discussed above. Conceptually, however, both of these include two or more different kinds of behavior. Trial-and-error strategies, for example, may begin with any type of coercive response. If the strategy appears to be successful, then the state will continue its behavior; if it appears to fail, then the state will select a new strategy. In principle, there can be five very different patterns of behavior: (1) a state remains highly coercive throughout, (2) a state begins with a low level of coercion and increases it as the challenger continues to insist, (3) a state begins with a high level of coercion and backs down as the challenger continues to insist, (4) a state uses a very low level of coercion throughout, and (5) a state fluctuates aimlessly in its level of coercion, unable to find a strategy that works. The first type I recoded as bullying, the second as firm-but-flexible, and the last three as appeasing. Similarly, stonewalling in principle can refer to two very different situations: (1) The defender may ignore the challenger's threatening actions, and the challenger is unable to accomplish its goals; or (2) the defender may ignore the challenger, and the challenger achieves its goals in the face of the defender's inaction. The first of these situations I recoded as bullying, the second as appeasement.[9]

Violation of a Legitimate Settlement

For each case in my data set, I determined whether the previous crisis had resulted in a legitimate dispute settlement.[10] Such settlements are of two types. "Explicit settlements" involve a treaty, armistice, cease-fire, and so on. The agreement may take the form of a formal document, public letter, or oral declaration, but the conditions are explicitly stated. "Tacit settlements" are mutual understandings by the adversaries that are neither written nor stated publicly. All other results of previous crises were coded as lacking a legitimate settlement. These outcomes also fall into two categories. First, in "imposed outcomes," one actor emerges as the clear victor in the crisis and imposes its desires on its adversary. The solution may or may not be couched in terms of a treaty settlement. I do not consider an imposed treaty a dispute settlement, since the agreement is not likely to be viewed as legitimate by the defeated party. A dispute settlement is differentiated from an imposed outcome by the presence of bargaining between the adversaries over the policies included in the settlement. If a settlement reflected no responsiveness to the preferences of one party, then I coded the outcome as imposed. Second, in a "stalemate outcome" the dispute simply fades without any change in the status quo or any agreement, imposed or otherwise.

After determining whether a previous legitimate settlement existed, I then identified the cases in which it was violated by the challenger. For each case I reviewed the relevant documents or statements and determined the substance of the settlement.[11] If the challenging state did not adhere to the terms of that understanding, either through the reinitiation of the crisis or at any time during the crisis, then I coded it as in violation of the settlement. In practice this process is less difficult than it may appear, since the central component of most of these settlements—both tacit and explicit—is the establishment of some policy (often a territorial boundary) that the parties agree not to attempt to overturn by force. Thus, the reinitiation of a crisis over the same issue itself generally constitutes a violation. For all crises in which no such settlement existed, I coded the challenger as not in violation. That coding also was used if a previous settlement existed but the actions of the challenger in the subsequent dispute did not violate its specific terms.

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[8] Having identified the move/countermove sequence of each crisis, I relied on Leng's (1989; Leng and Wheeler 1979) procedures, but my codings cannot be considered a duplication of the Behavioral Correlates of War (BCOW) data gathering effort (Leng 1989). In particular, BCOW used primary sources and multiple coders to test for intercoder reliability. Resource constraints forced me to rely on the ICB case summaries, secondary sources cited by ICB coders, *Keesing's Contemporary Archives*, and *Facts on File* in identifying the sequence of moves in each crisis.

[9] In terms of my data set, all the stonewalling cases were recoded as appeasement. I did not encounter any of the other possible patterns of behavior.

[10] Coding was based on the "Form of Outcome" variable in the ICB data set. I then reviewed each case myself to determine whether a settlement (1) was present, (2) was "legitimate," and (3) had been violated.

[11] In selecting primary and secondary sources, I relied on the bibliographies provided by the ICB data set.

Interaction of Bargaining and Settlement

The three possible defender bargaining strategies combine with the presence or absence of a previous settlement to define six possible actions by the defender: (1) appeasement without a previous settlement, (2) appeasement after violation of a previous settlement, (3) firm-but-flexible without a previous settlement, (4) firm-but-flexible after violation of a previous settlement, (5) bullying without a previous settlement, and (6) bullying after violation of a previous settlement. To avoid severe problems of multicolinearity in testing a curvilinear relationship combined with an interaction, I tested the first four hypotheses with a series of dummy variables. Using firm-but-flexible without a previous settlement as the excluded category, I include five dummy variables to capture each of the possible interactions of previous settlements and the defender's bargaining strategy.[12]

The Balance of Military Capabilities

My indicator of conventional military resources has three components to measure the capabilities available for the current dispute: (1) military expenditures, (2) military personnel, and (3) the ratio of expenditures to personnel.[13]

For each component I summed the capabilities of both the challenger and its supporters and the defender and its supporters. Then, again for each component, I computed the defending side's proportion of the total dyad's capabilities in the following manner:

$$\frac{\textit{Defender} + \textit{Supporting Capabilities}}{(\textit{Defender} + \textit{Supporting Capabilities}) + (\textit{Challenger} + \textit{Supporting Capabilities})}$$

Finally, I calculated the balance of conventional military capabilities for the crisis as the average of these three ratios.[14]

The Defender's Bargaining Strategy and the Balance of Military Capabilities

To test the hypothesis that the balance of military capabilities has a greater effect when the defender shows willingness to use force, I included the measure of balance of forces interacted with a dummy variable identifying when the defender used a bullying strategy.

Possession of Nuclear Weapons

I began my coding based on the ICB data set and referred to additional sources in order to fill in missing data.[15] I then created a three-point nuclear scale for each state (1 = no capability, 2 = rudimentary, 3 = developed). The essential difference between rudimentary and developed nuclear capacity is possession of a secure second-strike force. States with a rudimentary capability have nuclear warheads and the capacity to deliver them onto the territory of their adversary, but they lack sufficient warheads and/or basing capabilities to be assured of a second strike. Finally, I used this three-point scale to create a dummy variable marking first-strike capabilities for the defender. The defender is attributed first-strike capacity if its nuclear score is greater than the challenger's (coded 1; 0 otherwise).

Interests at Stake in the Dispute

Codings for this variable were based on the ICB data set. In particular, Wilkenfeld, Brecher, and Moser (1988) use a six-value ordinal scale to score the gravity of the value threatened for each state in the crisis. A low level of threat refers to crises that threaten the economic or social activities of the state. In the second category are political threats to the viability of the state's current regime or leadership. Next are threats to a state's territorial integrity. The fourth category involves threats to a state's influence with its adversaries or allies within the regional or international system. The "grave damage" category is exemplified by large casualties in war or mass bombings. Finally, a crisis may threaten the very existence of a state as a political unit or the survival of its entire population.

In my data set, if a crisis threatened more than one type of value, the most severe threat was coded. According to realist theory, threats to international influence should not be considered more serious than those to the political stability of the government in power or to the territorial integrity of the state itself (Waltz 1979). Thus, I reordered the categories to code international influence lower than governmental stability.

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[12] An alternative specification of the relationship would be to include the three-category coerciveness of the defender's bargaining strategy along with its square to capture the curvilinear effect predicted by hypothesis 1. The influence of international norms could be estimated by interacting each of these variables with the dummy variable marking the violation of a previous settlement. This specification of the model yields predicted effects nearly identical to those described here. Very high levels of multicolinearity, however, prevent the coefficients in that model from achieving statistical significance. The auxiliary R^2 for the normative variables in that specification exceed 0.99.

[13] The ratio of spending per soldier is a crude measure of troop quality. It is likely to capture the amount of resources spent on equipment, training, and support services for each soldier. For a more detailed discussion of this measure, see Huth, Bennett, and Gelpi 1992. The data are taken from the COW data set on military capabilities (Singer and Small 1993). For crises that occurred in the first quarter of a year, I used the previous year's data as a more accurate reflection of capacity. The capabilities of all parties involved in the crisis are discounted for the distance from each state's homeland to the point of conflict, using the procedure developed by Bueno de Mesquita (1981).

[14] One might hypothesize that the balance of military capabilities would have a nonlinear effect on the challenger's insistence on overturning the status quo. When the distribution of capabilities is extremely uneven, changes in the relative balance may have little effect on states' expectations of the outcome of a conflict. As the balance nears equality, however, small changes may have a substantial influence. I attempted to test this hypothesis by using a cumulative normal transformation of the balance of military capabilities. The transformed variable, however, correlated at .997 with the linear coding of balance of military capabilities and generated a coefficient and marginal effects that mirrored those from the linear coding. Thus, I retained the simpler variable in the analysis.

[15] The main sources consulted were Goldschmidt 1982 and Betts 1987.

Concurrent Involvement in Other Disputes

I coded this variable based on the COW data set on militarized international disputes (Singer and Small 1993).[16] The value is simply the number of militarized disputes in which a state was involved both during the current crisis and the six months preceding it.[17]

Behavior in Previous Disputes

Conceptually, this variable captures a state's willingness to use force in defense of its interests. In order to measure this willingness, I examined both a state's bargaining behavior in the previous crisis and its success in achieving its objectives. Specifically, I used a combination of the ICB variables, which identify the "centrality of violence" to an actor's bargaining strategy and the "content of the crisis outcome," to identify the party that was defeated in the previous crisis *and* that did not use force or only a minor amount to avoid this defeat. Such states are coded as having capitulated in the previous dispute (value of 1). Otherwise, a value of 0 was coded.

History of Previous Conflict

This variable is coded on the basis of the ICB data set, that is, the number of times that the challenger and defender were the primary disputants in international crises prior to the current one.[18] Since this variable reflects either the domestic political implications of engaging in conflict or the lessons leaders learn from a pattern of hostile interaction, I included all earlier crises between the challenger and defender between 1929 and 1979, regardless of whether the issues recurred in the current crisis and regardless of which state was identified as the previous challenger.

Change in the Balance of Conventional Military Capabilities

This variable is coded as the change in the proportion of dyadic capabilities attributable to the current challenger since the most recent crisis over the same issue. First, I determined the defender's proportion of the dyadic balance of military capabilities for the previous crisis in an identical manner to the procedure described above. Second, I subtracted the defender's proportion of dyadic capabilities in the current crisis from the value for the previous crisis. Thus, positive values on this variable reflect an increase in challenger capabilities between the previous and current disputes.

DATA ANALYSIS AND STATISTICAL RESULTS

For the statistical analysis, the ordered-probit approach was used. It is analogous to ordinary regression but is the appropriate tool for dependent variables with three or more ordinally ranked categories (Hanushek and Jackson 1977, King 1989).[19] As discussed earlier, the interaction between the defender's bargaining strategy and the violation of a previous settlement is represented by five dummy variables, with firm-but-flexible and no settlement as the excluded category. Recall that hypothesis 1 predicts a concave curvilinear relationship between the coerciveness of the defender's strategy and the challenger's insistence on overturning the status quo in the absence of a previous dispute settlement. This relationship is represented in the statistical model through the combination of two variables: appeasement with no settlement and bullying with no settlement. I expect to find a positive coefficient for both since, in the absence of a previous settlement, both should result in more challenger resistance than would a firm-but-flexible strategy. Hypothesis 2 predicts that if defenders are appeasing, then challengers will be more insistent if they have violated a settlement than if they have not. This effect will be reflected in a positive coefficient on the dummy variable for defender appeasement after a previous settlement that is substantively larger than the coefficient for appeasement with no settlement. Hypothesis 3 predicts that the challenger will yield if the defender's coercive behavior represents a punishment for the violation of a previous settlement. In other words, bullying will not have a significantly different effect on the challenger's response than would a firm-but-flexible strategy without a previous settlement. Consequently, the coefficient for this variable should be much smaller than the coefficient for bullying without a

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[16] The COW data set includes a number of cases of such low intensity that I did not wish to drain resources by using them. To count as a dispute, a conflict must last at least one week, and there must be some militarized action or threat of it by both parties to the conflict.

[17] Involvement in an interstate war counts as engagement in another dispute if the war lasted until at least one year before the beginning of the current crisis.

[18] Generally, of course, all cases will score at least a one on this variable. Some cases may be coded zero, however, if the challenger and defender were not the *primary* disputants in previous crisis. Such crises are properly coded as reinitiations, for my purposes, since the two disputants were participants. For this variable, however, I identify the number of times that these states have been primary disputants because both Huth (1996) and Leng (1983) suggest that the influence of previous conflicts should be stronger if the two states were the primary disputants over the most critical issues at stake.

[19] Because my dependent variable has three categories, the statistical model must estimate a constant term and two threshold boundaries between the categories. Since the dependent variable in an ordered-probit analysis has no metric scale, one of these values must be held constant while all other coefficients are calculated in relation to it. The statistical software accomplishes this estimation by assuming that the coefficient for the constant term is 0. The first and second thresholds are then estimated as parameters in the model along with those for the independent variables. The estimated thresholds between the categories appear in Table 1.

TABLE 1. Probit Analysis Predicting Challenger Response: Normative and Realist Variables

Explanatory Variables	Coefficient	Standard Error	Probability Level
Normative Variables			
Bully, No Settlement	1.73	0.70	<.01
Bully, Settlement Violated	0.61	0.65	.35
Firm/Flex, Settlement Violated	−0.26	0.47	.57
Appease, No Settlement	1.12	0.49	.01
Appease, Settlement Violated	1.98	0.59	<.001
Realist Coercion Variables			
Balance of Military Capabilities	−0.65	0.68	.17
Balance of Military Capabilities × Bully	−0.15	1.01	.44
Defender First Strike Capability	0.78	0.40	.05[a]
Defender Interests at Stake	0.13	0.08	.10[a]
Defender Number of Other Disputes	0.03	0.12	.41
Defender Previous Capitulation	0.45	0.39	.13
Number of Previous Disputes	−0.02	0.08	.39
Change in Balance of Military Capabilities	−0.99	1.45	.25
First threshold	0.33	0.59	
Second threshold	1.00	0.59	

Number of observations = 122
Number of observations correctly predicted = 82
Percentage correctly predicted = 67.2%
Proportional reduction in error = 28.1%
Initial log likelihood = −122.91
Log likelihood at convergence = −95.01
[a] Two-tailed test for significance. All other tests are one-tailed.

previous settlement, and it should not be statistically significant. Finally, hypothesis 4 predicts that a firm-but-flexible strategy following a violation will be at least as effective in persuading the challenger to yield as it would be in the absence of a settlement; therefore, the coefficient on this variable should be either zero or negative.

My expectations for the realist control variables are relatively straightforward and follow directly from rational coercion theory and the enduring rivalries literature. I expect negative coefficients both on the balance of conventional military capabilities and on the interaction between a bullying strategy and the balance of forces. The latter coefficient reflects the realist expectation that the effectiveness of military capability will depend on the perceived willingness to use it. Next, I expect negative coefficients on the defender's interests at stake and the defender's possession of a nuclear first-strike capability because both high motivation and nuclear advantage should make defenders more capable of forcing challengers to yield. Finally, I expect positive coefficients on the variables marking a previous capitulation by the defender, the defender's involvement in other disputes, the number of previous crises between the dyad, and the change in the challenger's proportion of dyadic conventional military capabilities; each of these factors should make challengers more insistent on overturning the status quo. The results of the probit analysis are presented in Table 1.[20]

The results strongly support my argument concerning the role of norms and dispute settlements. As predicted by both frameworks, increasing the coerciveness of the defender's strategy has a curvilinear effect on the challenger's insistence on overturning the status quo when no settlement has been violated. The positive coefficient on appeasement with no settlement indicates that the challenger will insist more firmly on an alteration of the status quo relative to the excluded category (firm-but-flexible, no settlement). The positive coefficient on the variable for bullying with no settlement indicates that highly coercive strategies in these cases will also make challengers highly insistent. These findings confirm the realist conventional wisdom concerning crisis bargaining, reflected in hypothesis 1. While both realism and my normative approach predict this curve, coercion theory cannot account for the sharp differences in the challenger's response to the defender's bargaining strategy when a previous dispute settlement is violated.[21] First, consistent with hypothe-

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[20] By using dummy variables, I can keep the multicolinearity among my normative variables reasonably low. Specifically, the auxiliary R^2 for bullying with a previous settlement and bullying without a settlement were approximately .84. The auxiliary R^2 for the other normative variables ranged from .49 to .62. The auxiliary R^2 for the balance of forces was .57, and its interaction with bullying strategies was slightly higher, at .84. The auxiliary R^2 for the other realist variables ranged from .10 to .30. Thus, multicolinearity cannot be forwarded as an explanation for the generally weak results on the realist variables. In addition, reestimation of this equation with the insignificant variables dropped yields no change in the substantive size of the remaining coefficients or their statistical significance.

[21] The objection may be raised that challengers respond to the punishment of a violation when the interests at stake are relatively unimportant but not when the stakes are high. This problem should be controlled to a significant degree by my inclusion of the defender's interests at stake in the equation, since challenger and defender stakes are strongly correlated. In order to be certain, however, I also included the seriousness of the challenger's interests at stake in my preliminary analyses. Since that did not alter the substantive or statistical significance of the normative variables, I removed it from the final equation.

TABLE 2. Marginal Effects of Normative Variables on Challenger's Response

Section A: Marginal Effects of Previous Settlements on the Challenger's Response, Controlling for the Defender's Bargaining Strategy

Change in Prev. Settlement	Change in *P* (Compliance)	Change in *P* (Compromise)	Change in *P* (Intransigence)
If defender appeasing:			
No to yes	−15.4%	−12.3%	+27.7%
If defender firm-but-flexible:			
No to yes	+9.2%	−3.8%	−5.4%
If defender bullying:			
No to yes	+31.4%	+10.6%	−42.0%

Section B: Marginal Effects of the Defender's Bargaining Strategy on the Challenger's Response, Controlling for Previous Settlements

Change in Defender Coercion	Change in *P* (Compliance)	Change in *P* (Compromise)	Change in *P* (Intransigence)
If no previous settlement violated:			
Appeasing to firm-but-flexible	+44.0%	−2.3%	−41.7%
Firm-but flexible to bullying	−55.4%	−5.3%	+60.7%
If previous settlement violated:			
Appeasing to firm-but-flexible	+68.7%	+6.2%	−74.8%
Firm-but-flexible to bullying	−33.2%	+9.1%	+24.1%

Note: Marginal effects were calculated by generating predicted values from the probit model while changing the values of selected variables and holding the others at their means or modes. The predicted values were transformed into probabilities that the outcome would fall into each catetory by summing the area underneath the cumulative normal distribution between the predicted value and each of the category thresholds.

sis 2, the coefficient on the dummy variable marking appeasement following a violation of a settlement is positive and almost twice the size of the coefficient for appeasement in the absence of a settlement. This result indicates that when defenders appease, challengers insist more on changing the status quo if this appeasement follows the violation of a settlement. Second, the insignificant coefficient on the dummy variable for bullying after a violation indicates that these cases are not statistically different from cases of firm-but-flexible with no settlement. Moreover, the coefficient for bullying after a violation of a previous settlement, while positive, is one-third the size of the coefficient for bullying in the absence of a previous settlement. This finding supports hypothesis 3, which predicts that challengers will be more compliant in the face of bullying when the defender's actions represent punishment for a violation of a previous settlement. Finally, consistent with hypothesis 4, the coefficient on the variable for firm-but-flexible strategies following a violation of a settlement is negative, but it is substantively small and not statistically significant. Thus, firm-but-flexible strategies may be slightly more effective in persuading the challenger to yield if they follow the violation of a settlement, but this effect is fairly minimal.

Because the effect of dispute settlements is spread across several variables in my model, I test the overall statistical significance of their influence by comparing the model in Table 1 to a specification which includes the defender's bargaining strategy but excludes its interaction with previous dispute settlements. A likelihood ratio test revealed that the data I observe are more likely to have been generated by a process which includes the effect of settlements. The χ^2 value for this test was 11.27 with 3 degrees of freedom ($p < .025$).

The substantive effects of dispute settlements on crisis outcomes are also quite large. These are summarized in Table 2. In Section A, I calculate the changes in the predicted probability of challenger intransigence, compromise, and compliance by varying the presence or absence of a violation for each of the defender's possible bargaining strategies. Section B then reverses this process by varying the defender's bargaining strategy while holding the presence or absence of a settlement violation constant.

Consistent with hypothesis 2, Section A of Table 2 demonstrates that when the defender is appeasing, the violation of a previous settlement by the challenger actually increases its insistence on overturning the status quo as compared to cases in which there is no previous settlement. Specifically, the first row states that the likelihood of intransigence in response to appeasement increases by 28% if the challenger violates a settlement. Conversely, the likelihood of compliance and compromise decreases by 15% and 12%, respectively, when the defender is appeasing following a violation. Previous dispute settlements have their most powerful effect on crisis bargaining when the defender uses a bullying strategy. In keeping with hypothesis 3, coercive behavior following the violation of a settlement is 42% less likely to elicit intransigence from the challenger as compared to the case of no previous settlement. Conversely, of course, compliance and compromise become more likely challenger responses to bullying when that behavior represents punishment for a violation. Table 2 shows that the probability of each of these outcomes increases by 31% and 11%, respectively. Realist theory cannot explain why the effectiveness of coercive bargaining strategies appears to depend so significantly on the normative context in which such tactics are used. Finally, as predicted by hypothesis 4, when the defender is firm but flexible, the presence or absence of a previous settlement has relatively little effect on the challenger's response. In either case the likelihood of compliance or compromise is high. Even so, the fact that the defender is punishing the violation of a previous settlement does have some effect. As Table 2 illustrates, when the defender punishes a violation with a firm-but-flexible strategy, the probability of challenger compliance increases by 9% over cases in which there is no previous settlement, and the probability of compromise and intransigence decreases by 4% and 5%, respectively.

Section B of Table 2 also documents the contrasting relationships between the defender's strategy and the challenger's response with and without a previous settlement. Here, we can see the concave curvilinear relationship develop as expected by hypothesis 1. First, with regard to the crises without a previous settlement, Table 2 shows that as the coerciveness of the defender's strategy increases from appeasing to firm-but-flexible, the probability of challenger intransigence drops by 42%. In contrast, the probability of compliance by the challenger increases by a substantial 44%. In fact, the effect of moving from an appeasing to a firm-but-flexible strategy reduces the challenger's insistence on altering the status quo so sharply that the probability of a compromise is reduced by 2% as the predicted outcomes shift farther toward the compliance category. Further increases in the coerciveness of the defender's strategy, however, quickly reverse this relationship. Specifically, defender bullying increases the likelihood of challenger intransigence by a very substantial 61%. Conversely, the move from firmness to bullying reduces the predicted probability of challenger compliance and compromise by 55% and 5%, respectively.

When the challenger violates a previous settlement, however, the relationship between the defender's strategy and the challenger's response is very different. Section B of Table 2 shows that as the coerciveness of the defender increases from appeasing to firm-but-flexible, the probability that the challenger will remain intransigent drops by a striking 75%. My normative model expected that this decrease would be sharper after a challenger violation, and, indeed, this drop is nearly twice as large as when there is no previous settlement. Conversely, this shift in defender strategy increases the probability of challenger compliance by 69%. This change is more than half again the size of the increase in the absence of a settlement. The probability of challenger compromise also increases by a modest 6%. Finally, contrary to my predictions, continued increases in the defender's coerciveness increase the probability of challenger intransigence somewhat after the violation of a previous settlement. But the rise is only 24%, or slightly more than one-third the increase that occurs without a violation. Similarly, while this coerciveness reduces the probability of compliance by 33%, that is slightly more than half the decrease when no settlement has been violated. Moreover, the shift from a bullying to a firm-but-flexible strategy *increases* the likelihood of compromise by 9%. In sum, bullying is not as effective as firmness in lowering challenger insistence after the violation of a settlement, but it is *much* more effective in response to a violation than it is when there is no settlement. Specifically, the probability of challenger intransigence in response to bullying with no previous settlement is 76%, but when bullying punishes a violation the probability of intransigence is only 34%.

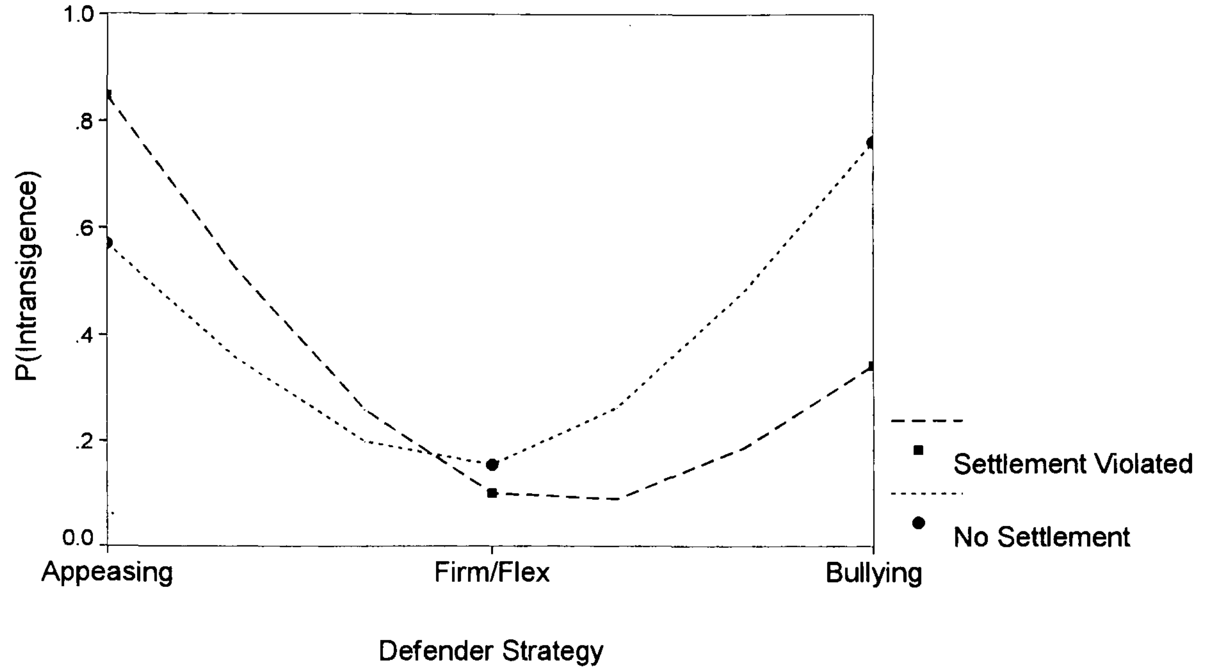
Finally, in Figure 2, I illustrate the effects described in tables 1 and 2 by plotting the predicted probabilities of challenger intransigence against the defender's bargaining strategy. In the figure I distinguish the predicted probabilities by whether or not the challenger violated a settlement. First, the clearly concave curvilinear form of the dotted line in Figure 2 captures the support I found for hypothesis 1. That is, with no previous settlement to violate, only firm-but-flexible bargaining strategies will be effective in preventing challenger intransigence. Second, the gap between the dashed and dotted lines on the left-hand side of Figure 2 indicates the support I found for hypothesis 2. Specifically, the dashed line is significantly higher when the defender is appeasing, which reflects a greater probability of challenger intransigence when that strategy follows a violation. Finally, the strong support I found for hypothesis 3 is reflected in the gap between the dashed and dotted lines on the right-hand side of the figure. Here, the dashed line is much lower, reflecting less likelihood of challenger intransigence in response to bullying after a violation.

Perhaps the clearest way to evaluate the predictions of my model is to compare the relationships described in figures 1 and 2. The fit between them is very close. In each case, challengers are more insistent on overturning the status quo in response to appeasing behavior if it follows the violation of a settlement, and they are much less insistent in response to bullying behavior in the same context. In fact, there appears to be only one significant difference between the data and the expectations of my model. Specifically, although it is apparent that bullying is a much more effective strategy when it represents punishment for a violation, even in this context it is less effective than the firm-but-flexible strategy. This finding is reflected in the slight curve that remains in the relationship between the coerciveness of the defender's strategy and the challenger's insistence on overturning the status quo even when a settlement has been violated. Since this finding was unexpected, any attempt to explain it must be recognized as post hoc. Nonetheless, one may postulate that while dispute settlements frequently act as persuasive referents and reputational constraints, these norms are not always persuasive. These deviant cases await further research in order to determine the conditions under which dispute settlements may lose their normative effect.

EVALUATING THE REALIST PARADIGM

While the data solidly support my normative bargaining model, the results for the realist control variables are mixed at best. First, consistent with Leng (1993)

FIGURE 2. Predicted Probability of Challenger Intransigence by Defender Strategy



Note: These probabilities were calculated by holding other variables at their means or modes.

and Huth (1988), the defender's bargaining strategy does have the curvilinear effect predicted by the realist model, but coercion theory cannot account for the influence of dispute settlements on the effects of coercion. Second, the coefficient for balance of military capabilities does have the predicted negative sign. The coefficient is substantively large, but it does not achieve conventional levels of statistical significance.[22] The substantive effect of the balance of military capabilities is given in Table 3, which reveals that a shift in relative capabilities from a 3:1 defender advantage to a 1:1 relationship increases the probability of challenger intransigence by 7.5%. An increase from 1:1 to a 3:1 challenger advantage raises the probability by another 7%. The coefficient for the interaction between the balance of military capabilities and the defender's bullying strategy is also negative, but it is substantively quite small and does not approach statistical significance. Thus, demonstrations of resolve do not appear to be important in creating a coercive effect for the balance of military capabilities.

The defender's capitulation in a previous dispute *does* affect the challenger's subsequent response. Although this coefficient does not achieve statistical significance ($p < .125$), it is much larger than for any of the previous three variables, and it is in the expected positive direction. Substantively, the influence of this variable is similar to that of the balance of military capabilities. If the defender capitulated in the previous crisis, then the probability of challenger intransigence increases by 18%.

None of the other variables in the realist model perform strongly. The defender's involvement in other militarized disputes, for example, has no effect on the challenger's behavior. Not only does the coefficient fail to approach statistical significance, but also it is extremely small. Similarly, neither the number of previous crises nor the change in the balance of military capabilities has any independent influence on the challenger's response. The coefficient for the number of previous disputes is very near zero and does not approach statistical significance, while the coefficient for the change in the balance of forces is actually in the wrong direction, though it is not statistically significant.

My findings concerning the influence of previous crises are somewhat puzzling, given the powerful support that Huth (1996) finds for their effect on the escalation of territorial disputes. A closer examination of his argument and my data set, however, can clarify the reasons for these differences. Huth maintains that previous disputes create domestic pressures against conciliation, but in testing his argument he does not control for the bargaining strategies used by the disputants. A quick look at my data set reveals that the coerciveness of the defender's strategy is positively correlated with the number of previous crises, although this correlation does not quite achieve statistical signif-

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[22] If the nonsignificant interaction term with the defender's use of a bullying strategy is dropped, then the statistical significance of this coefficient improves to $p < .09$.

TABLE 3. Marginal Effects of Selected Realist Variables on Challenger Response

Change in Independent Variable	Change in *P* (Compliance)	Change in *P* (Compromise)	Change in *P* (Intransigence)
Balance of military capabilities			
3:1 → 1:1	+7.4%	+0.1%	−7.5%
1:1 → 3:1	+7.9%	−0.9%	−7.0%
Defender previous capitulation			
No to Yes	−16.0%	−1.7%	+17.7%

Note: Marginal effects were calculated by generating predicted values from the probit model while changing the values of selected variables and holding the others at their means or modes. The predicted values were transformed into probabilities that the outcome would fall into each category by summing the area underneath the cumulative normal distribution between the predicted value and each of the category thresholds.

icance.[23] Thus, I suggest an indirect link between previous crises and the escalation of a current dispute. Escalation may occur if history persuades leaders to rely on bullying when such strategies are counterproductive (i.e., when no settlement has been violated). This explanation is consistent with Leng's (1983) findings that states learn realpolitik lessons about coercive strategies from repeated international conflict.

The effects of the final two variables in the coercion model are the most puzzling. Both the defender's possession of a nuclear first-strike advantage and the defender's interests at stake in the dispute appear to cause a sharp increase in the challenger's insistence on overturning the status quo. Since I had no theoretical expectations that could explain these results, any conclusions must once again be post hoc. Nonetheless, I examined the cases which appeared to be generating the positive coefficients in order to search for some tentative explanations.

First, with regard to the defender's possession of a first strike, its bivariate correlation with the challenger's response is near 0.[24] Only when the other variables in the coercion model are included in the analysis does it appear to have a positive effect. Moreover, the cases in which the defender has a first-strike advantage appear to fall into two groups. In the first set are crises in which both the challenging and defending states are Great Powers.[25] In these cases the defending United States and USSR did appear—either tacitly or explicitly—to hold the threat of nuclear weapons over their opponent. Certainly, the USSR and China considered the potential use of nuclear weapons by their adversaries. For these cases, the defender's nuclear first-strike capability *does* make the challenger more likely to yield, and in none of the cases did the challenger remain intransigent. In the second set are crises in which the defender has nuclear first-strike capability and the challenger is a minor power without nuclear weapons. In these cases a nuclear first strike was never seriously considered or threatened by the defender, and the challenger did not take the possibility of such use seriously.[26] Thus, as previous work has found, strong nuclear capability can serve as a powerful coercive tool against fellow Great Power adversaries (Huth, Gelpi, and Bennett 1993). Against smaller powers, however, nuclear threats may work but are almost never used.[27]

The process generating the estimated positive effect for salience of the defender's issue at stake seems somewhat different. The crucial factor appears to be the process by which crises entered my data set. As discussed earlier, any nonrandom selection of cases will necessarily affect the coefficients an analysis generates and will restrict the population to which any results can be generalized (Achen 1986). Reinitiated crises are clearly not a random set of international events. Many challengers may well be deterred from starting a dispute over an issue extremely important to the defender. They may be even more hesitant to reinitiate a crisis if the defender has already demonstrated strong resolve in protecting its interests. If a challenger chooses to do so, then it must have its own reasons for being very highly motivated to prevail. As a result, the effect of the defender's issue at stake on the challenger's behavior may be reduced in a data set that consists of such cases.[28] In fact, selection processes can have so pronounced an influence on the effects of some variables that their coefficients actually reverse sign (Fearon 1990, Gartner and Siverson 1996).

As was emphasized earlier, however, this nonrandom selection process and its effect on the estimated coefficients does not imply that the results are "wrong" or that the coefficients are "biased." The effects estimated by my analysis are correct for the population for which they were estimated: reinitiated crises. Once a dispute is revised, the presence or absence of a previous settlement in combination with the bargaining strategy used by the defender will have a much more important influence on the resolution of the crisis than will the other core realist variables I examined. These findings do not necessarily imply, however, that variables from realist theory do not affect whether a crisis is ever initiated or reinitiated. Although my population of cases may limit the generalizability of the results with regard to the realist variables, it also provides a particularly tough test for my normative variables. In particular, since I analyze crises which were initiated, resolved, and then reinitiated, the challengers are likely to be very highly motivated to overturn the status quo. It is particularly impressive that international norms can have such a powerful influence on these strongly motivated states.[29]

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[23] Specifically, the correlation coefficient is .14 ($p < .07$).

[24] The bivariate correlation coefficient is −.03, and it is not statistically differentiable from 0.

[25] My use of the term follows from Huth, Gelpi, and Bennett 1993 and Levy 1983.

[26] I would argue that the successful coercion by Britain of Guatemala over Belize was the result of Britain's firm-but-flexible strategy. No nuclear threats were made against Guatemala.

[27] This cannot be attributed to the fact that the defenders could prevail at the conventional weapons level, since many of them lost the encounters in which they chose not to make nuclear threats. A possible explanation for nonuse of nuclear power relates to the influence of international norms. See Paul 1995, Price and Tannenwald 1996, and Tannenwald 1997

[28] These selection effects probably also contribute to the positive coefficient for the defender's possession of a first strike and the insignificant result for the defender's involvement in other disputes. The selection effects seem particularly clear and pronounced, however, on the issues-at-stake variable.

CONCLUSION

The end of the cold war has sparked renewed interest in the influence of normative concepts on foreign policy behavior. My research builds on previous work by combining an attention to international norms with a recognition that bargaining and coercion remain central aspects of international politics. In developing this synthesis, I addressed two important problems with the institutionalist literature. First, empirical work has focused almost exclusively on international economic relations (Keohane 1984, Keohane and Martin 1995, Krasner 1983, Martin 1992). Scholars have frequently ignored the role of norms in security affairs because the costs of being exploited when a security-related norm is violated are potentially high (Jervis 1983) and because formal institutions are rare in security relations. I have shown that neither issue should prevent scholars from extending institutionalist analyses into security affairs. My findings indicate that international norms can influence security-related behavior. Moreover, I have shown that despite the paucity of formal international security organizations, the institutionalist concern for dispersing information about states' intentions and preferences and developing focal points for cooperation remains important in security relations. Thus, my analysis represents both a theoretical and empirical extension of institutionalism to the analysis of informal security norms. Second, scholars have had difficulty sorting out the potentially spurious relationship between shared norms and cooperative behavior. By focusing on the violation of norms, I have been able to separate the independent influence of shared norms from that of common interests.

My central finding is that bilateral security norms as established in dispute settlements help to legitimate coercive responses to a violation of the settlement. Specifically, I contend that settlements help create normative referents which alter interpretations of international behavior and generate a reputation for trustworthiness. In doing so, these settlements stabilize cooperation *even* between states that represent substantial security risks to each other. In the absence of normative referents for interpreting international behavior, coercive bargaining strategies are generally viewed by other states as threatening and generate fears of aggressive demands for concessions. Yet, conciliation can be viewed as a sign of weakness and is often exploited. Thus, in an entirely anarchic system, states must perform a difficult balancing act in order to achieve and maintain cooperative relations. International norms, however, stabilize cooperation by constructing referents for the interpretation of subsequent behavior and by investing state interests in a reputation for trustworthiness. Violators recognize coercive responses as legitimate punishment for their transgression and not as precursors to additional demands for concessions. Consequently, they are more likely to respond to coercive strategies by reestablishing their compliance with the norm, but this stabilization of cooperation comes at a price. In particular, the parties to dispute settlements must be willing to enforce the stipulations of their agreement, for my analysis shows that responding weakly to a violation leaves the defending state in a position even worse than if no previous settlement existed.

These findings represent a challenge to those realist scholars who argue that peace can only be maintained by military force and deterrent threats. Power and threats play an important role in determining behavior in international crises, but this realist view overlooks at least two important aspects of crisis bargaining. First, it neglects the fact that threats can be interpreted in a variety of ways. As a result, policymakers' normative referents—which shape the meaning of coercive strategies—have as important an influence on the outcome of international crises as do relative power and bargaining strategies. Second, in emphasizing the importance of a reputation for toughness, the realist paradigm ignores the value that states place on a reputation for trustworthiness. It is both by altering these referents and by engaging state reputations that international norms have a powerful influence on state responses to coercive behavior. These results represent a particularly strong challenge to the realist approach because my analysis focuses on militarized behavior in security-related disputes. Among all the issues of international politics, it is this kind of behavior that realism supposedly is best able to explain. Realists are correct to emphasize that ideas cannot stand alone in shaping state behavior; but the significant independent role that normative ideas can play in the resolution of security conflicts suggests that, while realism contains some important truths, it cannot stand alone and must be expanded to incorporate the role of normative referents in international politics.

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[29] It is possible that states which have agreed to a previous settlement are more "reasonable" or willing to negotiate than are other states, and hence they may be less likely to remain intransigent in subsequent crises. Yet, this cannot account for the fact that challengers are *more* likely to remain intransigent if the defender responds to the violation of a settlement with appeasement. Moreover, my reputational argument suggests that states will suffer additional costs by violating a settlement than they would if there were no settlement. Thus, challengers who choose to violate settlements may be *especially* motivated to overturn the status quo. I remain confident that my estimated effects of previous settlements represent the influence of norms on behavior, not simply the result of a self-selection process.

APPENDIX: REINITIATED INTERNATIONAL CRISES, 1929–79

Crisis Name	Year	Challenger	Defender	Chal. Resistance	Def. Coercion	Sett. Viol.	Previous Crisis Name	Prev. Year
Chaco II	1932	Bolivia	Paraguay	Intran	Appease	Yes	Chaco I	1928
Jehol Campaign	1933	Japan	China	Intran	Bully	No	Mukden Incident	1931
Bulgaria-Turkey II	1935	Turkey	Bulgaria	Compro	Firm/Flex	Yes	Bulgaria-Turkey I	1935
Ethiopian War	1935	Italy	Ethiopia	Intran	Appease	No	Wal Wal	1934
Marco Polo Bridge	1937	China	Japan	Intran	Bully	Yes	Jehol Campaign	1933
Anschluss	1938	Germany	Austria	Intran	Appease	Yes	Austrian Putsch	1934
Czech. Crisis	1938	Germany	Czech.	Comply	Firm/Flex	No	Remilitarize Rhine	1936
Changkufeng	1938	USSR	Japan	Intran	Appease	Yes	Amur River Incident	1937
Munich	1938	Germany	Czech.	Intran	Appease	Yes	Czech May Crisis	1938
Italy Threatens France	1938	Italy	France	Comply	Bully	Yes	Ethiopian War	1935
Czech. Annexation	1939	Germany	Czech.	Intran	Appease	Yes	Munich	1938
Memel	1939	Germany	Lithuania	Intran	Appease	Yes	Kaunas Trials	1935
Nomonhan	1939	USSR	Japan	Intran	Bully	Yes	Changkufeng	1938
Entry into WWII-Poland	1939	Germany	Poland	Intran	Bully	No	Danzig	1939
Entry into WWII-Lithuania	1939	Germany	Lithuania	Intran	Appease	Yes	Memel	1939
Soviet Occupation-Estonia	1939	USSR	Estonia	Intran	Appease	No	Entry into WWII	1939
Soviet Occupation-Latvia	1939	USSR	Latvia	Intran	Appease	No	Entry into WWII	1939
Soviet Occupation-Lithuania	1939	USSR	Lithuania	Compro	Firm/Flex	No	Entry into WWII	1939
Invasion Scandinavia	1940	Germany	Norway	Intran	Bully	No	Entry into WWII	1939
Fall of W. Europe	1940	Germany	France	Intran	Bully	No	Entry into WWII	1939
Closure Burma Road	1940	Japan	Britain	Intran	Appease	Yes	Tientsin	1939
Romanian Territories	1940	USSR	Romania	Compro	Appease	No	Entry into WWII	1939
E. African Campaign	1940	Italy	Britain	Intran	Bully	No	Ethiopian War	1935
Balkan Invasions	1940	Italy	Britain	Intran	Appease	No	Invasion of Albania	1939
Barbarossa	1941	Germany	USSR	Intran	Appease	Yes	Entry into WWII	1939
Maranon II	1941	Peru	Ecuador	Intran	Appease	Yes	Maranon I	1935
Pearl Harbor-USA	1941	Japan	USA	Intran	Bully	Yes	Panay Incident	1937
Pearl Harbor-UK	1941	Japan	Britain	Intran	Bully	No	Closure of Burma Rd	1940
Iran	1944	USSR	Iran	Comply	Bully	Yes	Occupation of Iran	1941
Azerbaijan	1945	USSR	Iran	Comply	Firm/Flex	Yes	Iran	1944
Turkish Straits	1946	USSR	Turkey	Comply	Firm/Flex	No	Kars Ardahn	1945
Indonesia Indep. II	1947	Netherlands	Indonesia	Comply	Firm/Flex	Yes	Indonesia Indep. I	1945
Indonesia Indep. III	1948	Netherlands	Indonesia	Comply	Firm/Flex	Yes	Indonesia Indep. II	1947
Sinai Incursion	1948	Israel	Egypt	Comply	Bully	No	Israel Independence	1948
Korea II	1950	USA	PRC	Intran	Bully	No	Korean War I	1950
Tel Mutilah	1951	Syria	Israel	Intran	Bully	Yes	Israel Independence	1948
Korea III	1953	PRC	USA	Comply	Bully	Yes	Korean War II	1950
Trieste II	1953	Yugoslavia	Italy	Compro	Bully	Yes	Trieste I	1945
Dien Bien Phu	1954	N. Vietnam	France	Intran	Bully	No	Invasion of Laos I	1953
Taiwan Straits I	1954	PRC	USA	Comply	Firm/Flex	Yes	China Civil War	1948
Costa Rica-Nica II	1955	Nicaragua	Costa Rica	Comply	Firm/Flex	Yes	Costa Rica-Nica I	1948
Gaza Raid	1955	Israel	Egypt	Intran	Appease	No	Sinai Incursion	1948
Pushtunistan II	1955	Afghanistan	Pakistan	Comply	Bully	No	Pushtunistan I	1949
Suez Nationalized-UK	1956	Egypt	Britain	Intran	Bully	No	Suez Canal	1951

APPENDIX: REINITIATED INTERNATIONAL CRISES, 1929–79 (continued)

Crisis Name	Year	Challenger	Defender	Chal. Resistance	Def. Coercion	Sett. Viol.	Previous Crisis Name	Prev. Year
Hungary Liberalized	1956	USSR	Hungary	Intran	Bully	No	Comm in Hungary	1947
Suez-Sinai-UK	1956	Britain	Egypt	Comply	Appease	No	Suez Nationalization	1956
Suez-Sinai-Israel	1956	Israel	Egypt	Comply	Appease	No	Gaza Raid	1955
Poland Liberalized	1956	USSR	Poland	Compro	Firm/Flex	No	Communism in Poland	1946
Nicaragua-Honduras	1957	Nicaragua	Honduras	Comply	Firm/Flex	Yes	Postage Stamp	1937
Berlin Deadline	1958	USSR	USA	Compro	Firm/Flex	Yes	Berlin Blockade	1948
Tunisia-France II	1958	France	Tunisia	Comply	Firm/Flex	Yes	Tunisia-France I	1957
Taiwan Straits II	1958	PRC	USA	Comply	Firm/Flex	Yes	Taiwan Straits I	1950
Cuba-Central Am. II	1960	Cuba	Nicaragua	Intran	Bully	No	Cuba-Central Am. I	1959
Pushtunistan III	1961	Afghanistan	Pakistan	Intran	Bully	No	Pushtunistan II	1955
Bizertia	1961	France	Tunisia	Comply	Appease	Yes	Tunisia-France II	1958
Berlin Wall	1961	USSR	USA	Intran	Appease	Yes	Berlin Deadline	1957
West Irian II	1961	Netherlands	Indonesia	Comply	Firm/Flex	No	West Irian I	1957
Goa II	1961	India	Portugal	Intran	Appease	Yes	Goa I	1955
Taiwan Straits III	1962	Taiwan	PRC	Comply	Firm/Flex	Yes	Taiwan Straits II	1958
Pathet Lao Off II	1962	N. Vietnam	USA	Compro	Firm/Flex	Yes	Pathet Lao Off I	1961
India-PRC Border I	1962	PRC	India	Intran	Bully	Yes	India-PRC Border I	1959
Ogaden I	1964	Somalia	Ethiopia	Comply	Firm/Flex	No	Ethiopia-Somalia	1960
Yemen War II	1964	S. Arabia	Yemen	Compro	Firm/Flex	Yes	Yemen War I	1962
Gulf of Tonkin	1964	USA	N. Vietnam	Intran	Bully	No	Vietcong Attack	1961
Yemen War III	1964	S. Arabia	Yemen	Compro	Firm/Flex	Yes	Yemen War-II	1964
Pleiku	1965	N. Vietnam	USA	Intran	Bully	No	Gulf of Tonkin	1964
Kashmir II	1965	Pakistan	India	Comply	Bully	Yes	Kashmir I	1947
Yemen War IV	1966	Yemen	S. Arabia	Compro	Bully	Yes	Yemen War-III	1964
El Samu	1966	Israel	Jordan	Intran	Bully	Yes	Israel independence	1948
Six Day War-Egypt	1967	Egypt	Israel	Intran	Bully	Yes	Suez-Sinai Campaign	1956
Six Day War-Jordan	1967	Jordan	Israel	Intran	Bully	No	El Samu	1966
Six Day War-Syria	1967	Israel	Syria	Intran	Bully	Yes	Tel Mutilah	1951
Cyprus II	1967	Greece	Turkey	Comply	Bully	Yes	Cyprus I	1963
Tet Offensive	1968	N. Vietnam	USA	Intran	Appease	No	Pleiku	1965
Prague Spring	1968	USSR	Czech.	Intran	Appease	No	Comm in Czech.	1948
Pre-War of Attrition	1968	Egypt	Israel	Comply	Bully	Yes	Six Day War	1967
Vietnam Spring Offensive	1969	N. Vietnam	USA	Intran	Appease	Yes	Tet Offensive	1968
War of Attrition I	1969	Egypt	Israel	Comply	Bully	No	Pre-War of Attrition	1968
Shat-Al-Arab II	1969	Iraq	Iran	Comply	Bully	Yes	Shat-Al-Arab I	1959
War of Attrition II	1970	Israel	Egypt	Compro	Firm/Flex	No	War of Attrition I	1969
Invasion of Cambodia	1970	N. Vietnam	USA	Intran	Bully	No	Vietnam Spring Off	1969
Cienfuegos	1970	USSR	USA	Comply	Bully	Yes	Cuban Missiles	1962
Invasion of Laos II	1971	USA	N. Vietnam	Intran	Bully	Yes	Pathet Lao Off II	1962
Vietnam Ports Mining	1972	N. Vietnam	USA	Compro	Firm/Flex	Yes	Invasion of Cambodia	1970
Uganda-Tanzania II	1972	Tanzania	Uganda	Compro	Bully	Yes	Uganda-Tanzania I	1971
Christmas Bombing	1972	N. Vietnam	USA	Compro	Bully	Yes	Vietnam Ports Mining	1972
Zambia	1973	Rhodesia	Zambia	Intran	Bully	No	Rhodesia's UDI	1965
Iraq Invasion Kuwait	1973	Iraq	Kuwait	Compro	Firm/Flex	Yes	Kuwait Independence	1961

APPENDIX: REINITIATED INTERNATIONAL CRISES, 1929–79 (continued)

Crisis Name	Year	Challenger	Defender	Chal. Resistance	Def. Coercion	Sett. Viol.	Previous Crisis Name	Prev. Year
Yom Kippur War-Egypt	1973	Egypt	Israel	Intran	Bully	Yes	War of Attrition II	1970
Yom Kippur-Syria	1973	Syria	Israel	Intran	Bully	Yes	Six Day War	1967
Cyprus III	1974	Greece	Turkey	Comply	Bully	Yes	Cyprus II	1967
Final Vietnam Offensive	1974	N. Vietnam	S. Vietnam	Intran	Bully	Yes	Christmas Bombing	1972
Cod War II	1975	Iceland	Britain	Compro	Firm/Flex	Yes	Cod War I	1973
Nouakchott I	1976	Algeria	Mauritania	Comply	Bully	No	Sahara	1975
Nagomia Raid	1976	Rhodesia	Mozambique	Intran	Bully	No	Operation Thrasher	1976
Mapai Seizure	1977	Rhodesia	Mozambique	Intran	Bully	No	Nagomia Raid	1976
Belize II	1977	Guatemala	Britain	Comply	Firm/Flex	Yes	Belize I	1975
Nouakchott II	1977	Algeria	Mauritania	Intran	Bully	No	Nouakchott I	1976
Ogaden II	1977	Somalia	Ethiopia	Intran	Bully	Yes	Ogaden I	1964
Rhodesia Raids	1977	Rhodesia	Zambia	Intran	Bully	No	Zambia	1973
Vietnam Inv. Cambodia	1977	Vietnam	Cambodia	Intran	Bully	No	Final Vietnam Off	1974
Chimoio-Tembue Raids	1977	Rhodesia	Mozambique	Intran	Firm/Flex	No	Mapai Seizure	1977
Chad-Libya II	1978	Libya	France	Compro	Firm/Flex	Yes	Chad-Libya I	1971
Lebanon Civil War II	1978	Syria	Lebanon	Intran	Bully	No	Lebanon Civil War I	1976
Sino-Vietnam War	1978	Vietnam	Cambodia	Intran	Appease	No	Vietnam Inv Cambodia	1977
Litani Operation	1978	Israel	Lebanon	Compro	Firm/Flex	No	Beirut Airport	1968
Chad-Libya III	1978	Libya	France	Compro	Firm/Flex	Yes	Chad-Libya II	1978
Cassinga Incident	1978	S. Africa	Angola	Intran	Appease	Yes	War in Angola	1975
Shaba II	1978	Angola	Zaire	Comply	Appease	No	Shaba I	1977
Air Rhod Incident	1978	Zambia	Rhodesia	Comply	Bully	Yes	Rhodesia Raids	1977
Beagle Channel II	1978	Argentina	Chile	Compro	Firm/Flex	Yes	Beagle Channel I	1977
Fall of Amin	1978	Uganda	Tanzania	Comply	Bully	Yes	Uganda-Tanzania II	1972
Tan Tan	1979	Algeria	Morocco	Intran	Firm/Flex	No	Sahara	1975
Raids on Zipra	1979	Rhodesia	Zambia	Intran	Appease	No	Air Rhodesia	1978
North-South Yemen II	1979	N. Yemen	S. Yemen	Compro	Bully	Yes	North-South Yemen I	1972
Raids on SWAPO	1979	S. Africa	Angola	Intran	Appease	Yes	Cassinga Incident	1978
Chad-Libya IV	1979	Libya	France	Compro	Firm/Flex	Yes	Chad-Libya III	1978
Goulimime-Tarfaya Rd	1979	Algeria	Morocco	Intran	Bully	No	Tan Tan	1979
Rhod Settlement-Zambia	1979	Rhodesia	Zambia	Comply	Firm/Flex	No	Raids on Zipra	1979
Rhod Settlement-Botswana	1979	Rhodesia	Botswana	Comply	Firm/Flex	No	Operation Tangent	1976
Rhod Settlement-Mozambique	1979	Rhodesia	Mozambique	Comply	Firm/Flex	No	Chimoio-Tembue Raids	1977
Raids on Angola	1979	S. Africa	Angola	Intran	Appease	No.	Raids on SWAPO	1979

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Political Participation in the Chinese Countryside

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The topic of grass-roots participation in China has acquired special interest in light of the local economic and political reforms introduced in the post-Mao period. This paper explores some major modes of participation, issues motivating participation, and the determinants of participation. The findings are based on interviews held in early 1990 with probability samples drawn from four countryside counties. The analysis emphasizes three modes of "autonomous" participation: cooperative actions, voicing opinions to cadres, and contacting representatives. Participants pursued selective and collective goods and were strategic in combining particular modes with particular problem areas. Determinants of participation included traditional resource model predictors but also unique predictors in the form of holding a second occupation, being a party member, and residing in a specific county. Local activists appear to be using new and traditional methods in moving toward more proactive, collective, and strategic forms of behavior.

Grass-roots participation in the People's Republic of China differs in fundamental respects from that found in more open societies. The existence of powerful governmental and party organs, the thorough penetration of society by the state, the scarcity of independent advocacy groups and other mechanisms for interest articulation, a shackled mass media, and the long absence of free and competitive elections have supplied a different meaning to the construct of participation. In particular, and in view of the top-down model often used to characterize the decision-making process in China, observers stress mass participation at the *implementation* stage rather than the *policymaking* stage—although contrary views also emphasize the importance of pressures and discontents percolating upward from the masses. Analysts also frequently characterize local participation in China as atomistic and informal, as being driven by particularistic rather than collective goals.

But these characteristics have not necessarily meant the absence of political participation as such. Just as some Sovietologists (e.g., Bahry and Silver 1990) have argued that participation in the former Soviet Union was less ritualized and disengaged and more heterogeneous than conventional wisdom would hold, so, too, some Sinologists (e.g., Burns 1988, Falkenheim 1987) have painted a somewhat more vibrant picture of local participation than that more conventionally portrayed. Moreover, some modes of local participation in Communist systems, including particularized contacting (seeking selective benefits), also appear in the larger behavioral repertories found in Western publics (e.g., Barnes, Kaase, et al. 1979b; Verba, Nie, and Kim 1978). Then, too, controversy often erupts at various levels over the implementation of policy in more participatory systems as well. China is surely not unique in this respect.

The topic of grass-roots participation in China acquires added interest in light of the systemic reforms introduced in the post-Mao period. Much of rural China underwent an economic transformation during the 1980s, with attendant changes in the standard of living and the quality of daily life. Rural areas, indeed, were in the vanguard of the movement away from a strictly controlled public sector economy toward a more privately oriented one. Given the intertwining of the Chinese economic and political systems, alterations also occurred with respect to the roles of government and party officials and, to some extent, the interaction of the citizenry with these officials. The upsurge in surplus labor, the proliferation of private markets, the switch to the household responsibility system, and the burgeoning of local industries created new sets of demands, expectations, skills, and relationships at the local level (e.g. Bernstein 1992; Oi 1990; Pei 1994; Shue 1988, chapter 4).

In addition to the consequences accompanying economic reform, other consequences emerged from reforms more explicitly political in nature (O'Brien 1994a; Oi 1989; White 1990, 1992). Drastic overhauling of the cadre system resulted in older, less professional cadres being eased out and replaced by younger, better educated ones (e.g., Manion 1993, Shen 1994). Township and town governments replaced the old communes, villages and villagers committees typically replaced production brigades, and village small groups replaced production teams. Competitive election of local leaders was encouraged, and the role of the local People's Congress was elevated (Jacobs 1991, O'Brien 1994b).

These mandated structural changes did not necessarily result in "real" changes in the exercise of power and the distribution of valued goods at the local level. Partly in recognition of this and in a move to strengthen and democratize village autonomy, an Organic Law of Villagers' Committees was passed in

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1987, with the first steps toward implementation being taken in mid-1988. Further implementation was delayed by the events of Tiananmen Square and then pushed with renewed vigor by 1990 (White 1992).

Without systematic longitudinal data it is difficult to establish just how much and what kind of change has occurred in mass political participation in the Chinese countryside during the reform era, or to assess with precision the effect of the various economic and political reforms that have transpired.[1] Due in part to the past constraints placed on certain types of research, most studies touching on mass public participation in China have either been of the sweeping, mass movement variety or, more relevant for present concerns, of the intensive, local case study type (e.g. Friedman, Pickowicz, and Selden 1991; Huang 1989; O'Brien 1996). The latter variety is quite useful for detailed, more holistic descriptions of particular locales, retrospective accounts of the changes that have occurred over time, and a chronicling of contemporary events.

But it is also important to conduct more extensive assessments of participation in the countryside in order to demonstrate the modes and patterns of participation; the issues and problems that motivate participation; and the characteristics that help account for variations in levels of participation. Doing so will provide evidence as to the consequences of a decade of reform, will help serve as baseline data for future assessments, and can situate local participation in China in cross-national perspective.

Three major points will be argued in this paper. One is that autonomous acts of political participation in the Chinese countryside are more frequent and variegated than is commonly supposed. They compare surprisingly well with Chinese urban areas and with national samples in more developed countries, even in the absence of strong, autonomous secondary associations. These rates of purposive, demanding activities reflect both traditional ways of exerting influence and the consequences of reform in the post-Mao period.

A second argument is that the issues and problems motivating participation are multidimensional by content and by objective. Interests are interests, and pluralism abounds, even in the Chinese countryside. The objectives of participatory acts extend beyond the particularistic goals of self-serving individuals that often are assumed when observing presumably rigid and tightly controlled polities. And participants use different participatory modes in strategic ways.

The third argument asserts that individual traits as well as contextual factors influence participation levels in ways quite compatible with participation models developed for more open societies. Indeed, in the absence of a strong civil society, the role of individual traits may be even stronger than in places with more extensive secondary associations. Peculiarities in the employment and party structures only serve to emphasize the point.

THE FOUR-COUNTY SURVEY

This paper draws on mass survey data that form part of a larger four-county study of local government and political economy in China. Selection of these four counties resulted from a combination of purposive and accidental/convenience criteria being applied to a list of approximately 30 counties meeting our requirements of variation and reasonable accessibility. They were purposely chosen so that they would differ in terms of economic well-being and diversification, proximity to large urban centers, population size, and province. The accidental/convenience aspect of their selection rests on the fact that gaining access to the counties depended upon local cooperation, which hinged in large part on the presence of contacts and connections there.[2] The counties are all rural or semirural,[3] and they range in size from around 275,000 to 750,000.

Due to our desire to perform multilevel analysis, including mass-elite relationships, the goal was to obtain an approximately equal number of mass public respondents in each county. Within each, a probability sample was drawn through a multistage process whereby five townships were first selected,[4] followed by selection of three villages within each township. Selection was based on probability proportionate to size (pps) at these two stages, with stratification by per-capita income at the township stage. Within each village an interval sample of individual respondents aged 18 years and older was drawn from the household listings, which included date of birth for each household member. Local officials imposed no restrictions or interference regarding the sample composition or access to respondents.

All interviews were conducted in February and March 1990 by well-trained advanced undergraduate and graduate students from Peking University, under faculty supervision.[5] These interviews took place in the respondent's domicile and averaged approximately one hour. The final number for the sample was 1,270, for an overall, unadjusted response rate of 77%—a figure very close to that of a roughly coterminous political survey in Beijing (Shi n.d.) and also quite comparable to response rates in most Western countries. Outdated household listings and respondent unavailability due to being away from the village accounted for the bulk of nonresponses.

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[1] Such difficulties are, of course, not confined to the topic of participation. See, for example, Rosen 1992 with respect to youth studies, and Entwistle et al. 1995 with respect to the effects of economic reform on the relationship between gender and work in rural areas. For a discussion of problems and procedures for conducting and evaluating survey research in China see, among others, Henderson 1993, Manion 1994, and Shi 1996.

[2] Permission had to be obtained not only from the State Education Commission but also local county officials. By agreement the counties—located in the provinces of Anhui, Hunan, Hebei, and Tianjin—must remain anonymous.

[3] Definitions of rural and nonrural vary in China. By the narrowest definition of nonrural, possession of an urban registration booklet, 82% of the population was rural as of 1987. By the most generous definition, a nonagricultural employment rate greater than half, 54% of the population was rural (Bauer et al. 1992).

[4] Technically, there were six townships; the county seats comprised a separate stratum and were selected with certainty.

[5] The fieldwork was originally scheduled for summer 1989 but was rescheduled due to the Tiananmen Square crackdown in June. Although Tiananmen had far less impact on the countryside than on urban areas, any effects still at work some nine months later most likely would have depressed reported rates of participation. Fieldwork supervisors received no indication of bias due to the use of student interviewers. Inspection of the marginal distributions based on the survey yielded little suggestion that the respondents were shading their answers either to please the interviewers or because they were intimidated by them.

The overwhelming majority of respondents were of Han ethnicity, originated from the countryside, and grew up in the county of current residence. About two-thirds came from families with a postliberation class designation of poor peasant. Nearly three-fourths classified their primary occupation as peasants, and slightly more than two-fifths had at least six years of education. The reported median household annual income was approximately 2,750 yuan.[6] A few more men (53%) than women are included in the sample.

In view of the way in which the counties themselves were chosen, the statistical results to be presented here can only be said to apply to the combined population of the four counties. However, these four are not rare or special cases, nor are they demonstration sites for economic or political experimentation. One is in the general orbit of a large city, but none is in the heavily marketized southern coastal area. More important, the scarcity of data of the sort used here argues for establishing some baselines for subsequent studies and for providing some rough comparisons with patterns found in other countries. Finally, the quantitative findings based on this inquiry can be compared with what has emerged from more qualitative studies of Chinese village politics in the reform era.

HOW MUCH AND WHAT KIND OF PARTICIPATION

While recognizing that local political participation can and does assume different guises in China as compared with Western systems, we nevertheless framed our questions to be compatible with those that have a lengthy lineage in Western surveys. Table 1 contains the wording of the five questions and the proportion of the sample engaging in each of the five acts.[7] The sampling design of the study should be kept in mind when interpreting these proportions. Although respondents *within* each county had a roughly equal chance of being selected, the probability of selection *across* the four counties was unequal because of their difference in population. Therefore, the frequency distributions are based on the sample weighted to reflect population size.[8]

TABLE 1. Frequency of Political Participation Acts in Four Rural Chinese Counties

Measures of Participation	% Yes
A. Have you attended a local party meeting recently?	8
B. Have you ever worked with other people, including family and relatives, to try to solve some local problem?	12
C. Have you ever written a letter to a cadre or offered an opinion or suggestion to a cadre?	14
D. Have you ever contacted a delegate to the county people's congress or township people's congress or a member of the village council?	24
E. Have you attended an all-village meeting recently?	47
Summary participation score	
None	41
One	31
Two	16
Three	8
Four–five	4
Only demanding activities (B,C,D)	
None	66
One	22
Two–three	12

Note: Data are weighted; N = 1,267.

Two activities involving attendance at meetings anchor these distributions. Going to local party meetings is the least frequent activity, engaged in by about one in twelve, while going to all-village meetings is the most frequent, cited by nearly one in two. Party members are undoubtedly under heavy pressure to attend party meetings, and nonmembers ordinarily are prohibited from attending. Village meetings, by contrast, are usually open to all, often serve as venues for expressions of support, and have a social as well as political function. Attendance also tends to be strongly encouraged, though with varying rates of success, by local cadres.

Sandwiched in between these two extremes are three activities that are much more demanding, purposive, and "autonomous" (Burns 1988) than sitting in on village sessions and much more accessible than attending party gatherings. They also typically require personal initiative rather than reflect mobilization by local cadres. About one in eight respondents reported working with others to solve a local problem, a form of cooperative or collective behavior that typically (though not necessarily) would involve public authorities and agencies in some fashion.[9] It is also a demanding one in that interest groups and voluntary organizations are either rare or under state and party control. Cooperative relationships have to emerge outside these frameworks. Cooperation also may involve going beyond the family and clan, moves not undertaken lightly in the tightly knit social structure of the countryside.

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[6] The exchange rate with the U.S. dollar at the time of the study was about 7:1. Comparing local and national statistics is problematic, but the four counties taken together resemble the total rural population with respect to the key indicators of income and education. Whereas the 1990 *per-capita* income for all rural residents was 686 yuan, the official figure for the four counties taken together was approximately 663, ranging from 483 to 769 across the four counties. As for education, 43% of all residents age 15 and older had gone beyond six grades as of 1990, compared with a survey-generated figure of 35% in the four counties. (National figures are taken from the *State Statistical Yearbook of China, 1994*, 64, 276.)

[7] Noticeably absent from this list are electoral activities. Because implementation of electoral reform was proceeding unevenly during the preparation of our project and, indeed, was put on hold after Tiananmen Square, we only asked a question about *interest* in local elections. For a resourceful use of data from the elite and mass public portions of the present study that reveals the importance of the emergent electoral connection in village politics see Manion (1996).

[8] A scaled weighting scheme was used such that the weighted sample size, which takes into account both county size and response rates, is the same as the unweighted sample size.

A slightly larger fraction reported writing or offering an opinion or suggestion to a cadre, which might be termed voicing behavior.[10] Although the relevant question was worded rather innocuously, it seems likely that the respondents viewed such activities with some seriousness. To register an opinion with a cadre may mean going on record and places the voicer in a potentially vulnerable situation. It also requires, for written voicing at any rate, a degree of literacy and articulateness that many peasants do not have.

Finally a considerable proportion, nearly one in four, reported that they had contacted a delegate to a local people's congress or a member of the village council.[11] There are three probable reasons for this substantially higher rate of activity. First, the delegates and council members are fairly numerous; with more targets, the opportunity structure is richer. Second, although delegates and council members may have connections, they are not necessarily cadres, so the concern about building ill will and possible recriminations would be lessened. Third, delegates and council members are in fact "supposed" to represent the people to their respective councils and congresses. Delegates in particular appear to function in a remonstrative capacity on behalf of their constituents (O'Brien 1994b). To the extent that the concept of popular representation has meaning in China, delegates and council members would be seen as legitimate channels of influence efforts.

A simple additive index (bottom, Table 1) shows that about two-fifths of the sample reported having performed none of the five listed behaviors. The numbers dwindle steadily, with less than one in twenty engaging in four or more. Overall, about one-third engaged in at least one of the more demanding, initiatory behaviors of cooperative efforts, voicing, and/or contacting. Most of this activity was of recent vintage; at least four-fifths of the reported dates fell within the past two years. Although this recency hints at the salutary consequences of political reform, the absence of baseline data and the tendency of survey respondents in general to report the latest instance (e.g., Rosenstone and Hansen 1993, 67–9) make such inferences problematic.

Are these small or large proportions, and what do they mean? A partial answer to the second question will come by examining the nature of the three more demanding behaviors, as described below. As for the absolute size of the proportions, it is instructive to compare them with results from within and outside China, even though the present findings are based on only four counties. A large-scale study of participation in urban Beijing in late 1988 and early 1989 (Shi n.d., chapter 3) found that 16% of the sample claimed to have worked with others to solve social problems in the past five years; 12% had written letters to government officials; and 9% had persuaded others to go to campaign meetings for deputies. Virtually all other proportions, aside from voting and activities directly related to the work unit, were also less than 20%. Such figures are not all that dissimilar to those appearing in the four-county survey, especially since the Beijing study occurred during a relatively liberal period and prior to the crackdown of the prodemocracy movement, whereas the four-county survey occurred within a year after the crackdown.

Comparisons with other countries present all kinds of difficulties, including those of instrumentation, timing, and sample composition. While it would be advantageous to make direct cross-country comparisons at similar stages of economic and political transition, the lack of comparable indicators severely limits that undertaking. A conservative approach is to make comparisons with national surveys whose participation questions served as the models for those in the present study. This restriction means that most of the comparison countries are Western democracies and that urban areas are included in the samples, factors that ordinarily would be expected to result, if anything, in higher figures for the comparison countries.[12]

Confining the analysis to the three more demanding activities shows that the Chinese sample trails nearly all other countries with respect to rates of cooperative, group-based efforts to solve problems and address issues, thus echoing the common observation about the weakness of civil society in China. With respect to the other two forms of demanding behavior, however, the Chinese sample fares much better, being ahead of some countries and behind others in terms of voicing and contacting, respectively.[13] Although comparisons with current and former Communist regimes are lim-

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[9] As will be demonstrated in Table 2, about three-fifths of actions under this mode involved agriculture, infrastructure, and general economic development—arenas that almost necessarily embrace interaction with officials. A substantial proportion of the social and human relations problems cited also appear to require going beyond private resolution.

[10] Some of these actions probably took the form of a letter of complaint, one of the more common types of local participation. For a much more elaborate and textured treatment of participation as applied in the urban context of Beijing, see Shi n.d.

[11] Some small portion of this reported contact could have been initiated by the delegate instead of the respondent, but the wording (*da guo jiaodao*, "ever contacted") clearly implies initiation by the respondent.

[12] Projects consulted included surveys in (1) Austria, India, Japan, the Netherlands, Nigeria, the United States, and Yugoslavia conducted in the late 1960s and early 1970s (Verba, Nie, and Kim 1978); (2) Austria, Finland, Great Britain, Italy, the Netherlands, Switzerland, the United States, and West Germany in the mid-1970s (Barnes, Kaase, et al. 1979a, b); (3) the Netherlands, the United States, and West Germany in the early 1980s (Jennings, van Deth, et al. 1989a, b); and (4) the American General Social Survey of 1987.

[13] The Chinese figures for attending all-village meetings are much higher than what would be found in most other countries. The higher rate is "artificially" inflated to the degree that attendance is prompted by the prospect of strong negative or positive sanctions from local officials.

TABLE 2. Issue Domain by Mode of Participation in Four Rural Chinese Counties

Issue Domain	Collective Action	Voicing Opinion	Contacting Delegate
Agriculture	29% (1)	20% (2.5)	17% (3)
Social welfare and human relations	27 (2)	20 (2.5)	16 (4)
General economy and enterprise development	15 (4)	26 (1)	13 (5)
Personal economy	5 (6)	10 (5)	25 (1)
Government and party affairs	6 (5)	12 (4)	20 (2)
Infrastructure	17 (3)	7 (6)	4 (7)
Miscellaneous village and everyday problems	1 (7)	4 (7)	5 (6)
Total	100%	99%	100%
Number of cases	151	177	302

Note: Number of cases = number of responses. Up to two responses were coded for each question. Column percentages may not equal 100 due to rounding. Parenthetical entries represent the rank order of the issue domain within the participation mode. The χ^2 value for all modes by issue domain is significant at the .001 level, as are the χ^2 values for the three cross-mode comparisons.

ited, a fairly direct comparison can be drawn with results from a national survey of the former Soviet Republic of Estonia (Brady and Kaplan 1995). The incidence of Estonians working with others to solve a community problem (10%) was roughly the same as in the Chinese survey, whereas the rate of contacting (about 12%) was considerably below the Chinese rate.[14]

It also should be noted that the individual level resources normally associated with political activity are much lower in China than in Western countries. Investing (statistically) the four-county respondents with as much education as those of Western subjects, for example, would substantially elevate their participation levels, ceteris paribus, as will become evident in the analysis to follow. Keeping in mind the rough-and-ready nature of the comparisons, the rate of countryside participation in the three more demanding modes is, rather surprisingly, not that distant from norms within and outside China.

THE SUBSTANCE OF PARTICIPATION

What was this citizen participation all about? What kinds of issues and topics prompted these autonomous actions? Different interpretations about the meaning of local participation arise depending on whether the issues simply reflect personal needs and grievances or more extended, communal concerns. The former are not unimportant, but the latter imply more concerted action and collective rather than particularized objectives. Follow-up, open-ended questions revealed a great range of topics that varied substantially according to the *type* of action initiated. These are consolidated into the following seven major categories, presented in descending order of the proportion of all responses that fell under each heading. Typical issues are listed for each.

Agriculture (21%): fertilizers, pesticides, fuels, irrigation, afforestation, and grain sales.

Social welfare and human relations (20%): education, morals, marriage, birth control, and community relations and disputes.

General economy and enterprise development (17%) *and personal economy* (16%): The former involve village or other collectivities, such as enterprise development; the latter refer to the individual or family, such as personal hardship, land distribution, and housing.

Government and party affairs (15%): party and government functioning, cadre behavior, and elections.

Infrastructure (8%): mainly public power and transportation.

Miscellaneous village and everyday problems (1%): rather vague, general references to village life and problems.

Given the agrarian nature of these counties, the emphasis on agriculture is not surprising. As recently as a decade ago it undoubtedly would have been much greater, and part of the change is due to the increased concern with various local enterprises that do not directly involve farming. Perhaps the only other remarkable feature about the overall distribution is the prominence of social welfare and human relations problems. There has been such a tendency to focus on the purely economic side of village life in the reform era that it is easy to forget the traditional roles played by local Chinese leaders in resolving conflicts and tending to other matters of general welfare (e.g., Huang 1989).

A fuller appreciation of the meaning and implications of these issue domains can be gained by looking at their variations in frequency according to participatory mode. Although the problems inciting action could be addressed through each of the three modes, each is differentially associated with particular kinds of problems ($p < .001$ using the χ^2 test). Table 2 presents the distributions of the seven categories by mode of participation. Each *column* shows the total proportion of all responses in that mode devoted to a particular problem area and the rank order of that problem area within each mode. Illustratively, 29% of all cooperative actions, 20% of all voicing actions, and 17% of all contacting behaviors addressed agricultural issues; the corresponding rank of agricultural issues was 1, 2.5, and 3 within each mode.[15]

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[14] The rate of contacting near the end of the Brezhnev era as reported by respondents in the Soviet Interview Project was 19% (Bahry and Silver 1990, 830), though this figure is probably inflated due to the social class bias represented by the sample of emigrants.

Cooperative mode problems are dominated by agricultural and infrastructure concerns, on the one hand, and social welfare and human relations, on the other. Many agriculture and certainly most infrastructure problems affect a range of people beyond the immediate household, thus making cooperative action a logical avenue. Similarly, many of the social welfare and human relations problems involve collective issues, such as public security, educational facilities, and rancorous conflicts, which lend themselves to group action.

When using the mode of voicing an opinion to a cadre, these local residents most often mentioned general economic and enterprise development issues. Indeed, "voicers" were motivated about twice as often by this topic as were users of the other two modes. Part of the transformation of the countryside during the reform era has been a virtual explosion of local industries and enterprises. As Oi (1990) and others have argued, local governments tend to become corporate managers in areas where these developments have proceeded the farthest. In addition, the household responsibility system has spawned groups of households that contract to produce a specialized product. Government, party, and enterprise cadres have become vital, if sometimes resented, actors in village and township economic development. Voice mode participants also emphasized agriculture and social welfare and human relations problems.

Finally, contacting officials, easily the most frequently practiced mode, is distinctive on two counts. First, people using this mode emphasized far more than did others the domain of government and party affairs. Occupying a central place in these references, despite an array of topics, was the theme of elections, which accounted for about two-fifths of all mentions. These contacts most likely included contests in which the delegates and council members were themselves involved. Based on these references, local elections appear to be acquiring high salience in the political life of the countryside. Significantly, when government and party affairs appeared under the other activity modes, elections were scarcely mentioned at all.

Another distinctive feature of the contacting mode is the degree to which it was used to communicate about personal economic woes and desires. Requests for intercession, favors, permission, and relief of various sorts characterize these contacts. Personal and familial hardship and economic matters loomed large in these communications, as did problems with land distribution and housing as well as vague references with implicit economic content. One hears in these transactions echoes of clientelism and political fixers.

It is precisely these kinds of transactions that lead to the characterization of local politics in China as being narrow and particularistic, in much the same way that some observers portrayed participation in the former Soviet Union before reform (e.g., DiFranciesco and Gitelman 1984). There is much truth to that depiction. Yet, even for the contacting mode of participation, such transactions constitute a distinctive minority of motivating issues, and counting *all* the social and human relations category as particularized contacts still leaves such interactions below 50% of all mentions. Personal grievances and requests appear to account for even less of the total range of motivating topics under the other two modes. Thus, local acts of participation have complexity and cannot be described as merely reflecting the pursuit of personal gain and satisfaction.

WHO PARTICIPATES?

The determinants of grass-roots participation in democratic societies are reasonably well charted, due in great part to the enormous amount of empirical research that has been carried out. Systematic variations according to personal characteristics and structural features have been recorded, with the individual resources and mobilization models being widely applied, either explicitly or implicitly. This body of knowledge is based on the cumulative effect of individual country as well as multicountry studies (e.g., Barnes, Kaase, et al. 1979a; Jennings, van Deth, et al. 1989a; Powell 1982; and Verba, Nie, and Kim 1978). Because of the lower centrality of mass publics in the decision-making process and the scarcity of systematic research, less is known about such determinants in nondemocratic and transitional societies.

Our expectations for the China project were guided in part by these studies of other systems as well as factors peculiar to the Chinese situation. The reason for emphasizing personal resources is that the modes of participation being analyzed here include those also employed in many non-Chinese settings. These are not activities of a patriotic or ceremonial nature orchestrated by the party or government. Moreover, the weakness of voluntary organizational life in China may well serve to emphasize the influence of personal traits. Thus, it seemed reasonable that some of the same determinants of participation would hold here as in more open societies. At the same time, unique features of the Chinese situation demanded attention, including the ubiquitous role of the Communist Party and a decade of remarkable economic reform and growth.

The Correlates of Participation

Social Stratification. Three likely correlates of participation—sex, education, and age—signify a person's location in the social stratification system of virtually any culture. That place in the system often carries with it certain skills, resources, rights, and expectations that are highly relevant for political participation. A raft of research around the world has demonstrated that, by most standards, men are more politically active than women, especially in terms of more demanding behaviors. In general, this gap is lower in the more "advanced" societies and among higher socioeconomic strata. For purposes of analysis, sex is coded here as 0 = female, 1 = male.

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[15] Because the lead-in questions dealt with mode of action rather than type of issue or problem, it would be imprudent to analyze these data by using the type of issue or problem as the percentage base.

Similarly, education is usually a strong predictor of most types of participation, although such structural properties as strong secondary groups can help overcome educational deficits. In this study, education is operationalized as the absolute number of school years completed (0–13+).[16]

Age has a more variable relationship to participation. The young typically are more likely than older people to engage in unconventional behavior, and the middle-aged tend to be more participative than younger and older people with respect to more traditional forms of activity. By most standards the political actions addressed here fall under the traditional heading. Due to the typical curvilinear pattern found by age, two age variables are used—absolute age and age squared. The latter takes into account the expected parabolic shape of the relationship.

Occupational status also is associated with participation, though its overlap with education and income (as in the present case) often makes its analytic use problematic. One particular feature in the Chinese countryside, however, warrants the inclusion of occupation as a possible predictor of political activity, namely, second occupations. These are not peculiar to the reform era, but the number and variety of nonagricultural employment opportunities grew as the market economy expanded in rural areas during the 1980s. Village and township enterprises and household businesses and sidelines, along with their supporting services, began to flourish.

To take advantage of these opportunities and perhaps as a hedge against future difficulties, a number of peasants took a second job while continuing—at least nominally—their work as peasants. Some 22% of the sample claimed to have a second occupation; the great majority of this subset (72%) listed peasant as their first occupation, while 13% listed worker. A plausible expectation is that rural people with a second occupation will be connected to more local networks, upwardly mobile, willing to push hard for their interests, and in expanding sectors of the economy where the rules of the game are still in flux.[17] It is precisely under such conditions that initiatory participation may prosper. For purposes of analysis, this variable is scored 0 (no second occupation) and 1.

Political Stratification. China is also characterized by an enormously significant *political* stratification system, the distinctions between cadres and noncadres, party and nonparty members, and center and periphery being the most salient features. Members of the Communist Party have unusual access to information, influential individuals, and experience in "working the system." They are well situated to engage in the kinds of initiatory activities that may seem daunting to the ordinary citizen. And even though economic and cadre reform caused temporary dislocations and new kinds of stresses, several observers have argued that local cadres and party members have learned to cope with and profit by these transformations (e.g., Brugger and Reglar 1994, chapter 3; Oi 1990; Pei 1994, chapter 3).

About 7% of the respondents identified themselves as party members, a slightly higher proportion than the estimated national rural figure of 5% (Pei 1994, 110, 228). For purposes of analysis, party members are coded as 1, the remainder as 0. Lest it seem tautological to include party membership as a determinant of participation, it should be noted that only one-fourth of the party members classified themselves as cadres. It is also important to note that party membership proved to be an inconsequential predictor of participation in the Beijing survey (Shi n.d.). As demonstrated below, further steps were taken in the analysis to clarify the relationship between membership and activity.

Political Attitudes. Another cluster of determinants are more sociopsychological in nature. One trait is subjective civic competence, the feeling that one has the skills and resources to take some sort of action to secure a preferred outcome. Implicit in such feelings is the perception that points of access are available for the exercise of these skills and resources. While this perception is quite common in open, democratic systems, it is more problematic in China's more controlled political setting. Civic competence was assessed by analyzing the responses to a traditional open-ended question, adapted to the Chinese site: "If you heard that leaders of this village/town were considering a measure that you thought unjust or harmful, what do you think you could do about it?" Respondents who averred that they could take some sort of action (41%), however trivial, were coded 1; all others, including "don't know," were coded 0.

Political action can be triggered when expectations about local services, conditions, and the general quality of life are not met, especially when the state is believed to affect the distribution of both tangible and intangible goods that people value (Sniderman and Brody 1977). In a society heavily penetrated by the state, as in China, the blame for these perceived shortfalls and shortcomings is especially likely to fall on government and party. Assuming the availability of channels for relief and response, the expectation is that the most dissatisfied people will be the most likely to seek relief.[18]

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[16] No particular gains were achieved by using a series of dummy variables to mark off various stages of completion in the Chinese educational system. Consequently, the variable is treated in a continuous fashion. Although there is a moderately strong negative relationship between age and education ($r = -.54$), it does not pose a serious problem of multicolinearity.

[17] Second jobs varied enormously. Substantial categories included peasant, worker, vendor, small shop operator, craftsman, transport business, and agricultural household sidelines. A second occupation is not significantly related to total family income, although those with second jobs claimed to contribute more ($r = .21$). They were also more often male ($r = .24$) and modestly better educated ($r = .17$).

[18] In the absence of regularized channels, they may resort to rancor-

Two indicators of dissatisfaction and frustration are used here. One references the number of *personal* problems and is based on answers to this question: "We are interested in learning about some of the difficulties people face in daily life. Could you tell us what difficulties you face in your daily life?" Respondents were scored according to whether they mentioned none (44%), one (34%), or two or more (22%) such difficulties.[19] A second measure captures perceptions about *local* problems. Respondents were presented with a listing of thirteen problem areas and asked to indicate which ones constituted local problems. The number of affirmative answers was summed to provide an additive index; the mean score was 3.9.[20]

Results and Interpretation

Although there are some differences in the correlates of participation according to the particular mode, for present purposes the three demanding modes (cooperative action, voicing, and contacting) have been combined into the cumulative index presented in Table 1. As anticipated, higher participation was significantly associated ($p < .001$) at the bivariate level with being male ($r = .25$), having more education (.29), having a second vocation (.22), feeling politically competent (.22), having more personal problems (.11), perceiving more local problems (.24), being a party member (.27), and being neither young nor old (determined by scatterplots and tabular analysis). These bivariate relationships are of intuitive and descriptive interest, all the more so because of the relative dearth of such materials based on surveys in China. Still, they reveal little about the staying power and precise contribution of any given determinant when other determinants are taken into account. To achieve these goals, multiple regression analysis (ordinary least-squares) was employed.

The results to be reported are based on three equations. The first employs all eight predictors except party membership. Even though one-third of the party members stated they had performed none of the three initiatory acts, it can be argued that engaging in demanding political activities is part of "the job description" rather than a more autonomous, voluntary behavior. Thus, a purer test of who participates excludes that variable from the analysis. Yet, it also can be argued that being a party member is associated with one or more of the other predictors and that these may mask the influence of party membership. One way to determine this is to present the results without including party membership and then estimate the second equation with its inclusion.

The third equation introduces a county-level variable. One county, which will be called County B, had a considerably higher participation rate than did the other three—a mean score of .70 on the participation index compared with means of .39, .33, and .32 for the other counties.[21] Two explanations are possible. First, individual-level characteristics associated with participation are higher in County B than in the other three counties. When aggregated, these individual-level differences produce county differences. Second, systemic or environmental effects may be at work. These could take the form of such factors as the receptivity of party and government officials to initiatory acts, speedy implementation of political reform measures (e.g., the Organic Village Law), historical traditions regarding participation, structural changes in civil units, marketplace forces, or acts of nature.

It is very difficult to choose among the various systemic explanations, but it is possible to make a reasoned choice between the larger compositional and systemic alternatives. Introducing a dummy variable to represent the effects of county (County B = 1, the remaining counties = 0) while simultaneously controlling for known individual-level determinants should reveal whether there is something unique about County B, a uniqueness which arguably represents a systemic or environmental property of the county. By the same token, introducing the county effect into the analysis offers a test as to the robustness of the individual-level traits.[22]

Turning first to the results without the inclusion of party membership and residence, it is abundantly clear from Table 3 that sex, education, age, age squared, a second job, local problems, and civic competence are all moderately to strongly related to participation, even when controlling for the effects of one another. Several of these associations bear a marked similarity to results from Western surveys, results that usually would hold in rural as well as urban areas. Sex, education, and civic competence are almost invariably related to most forms of participation in other countries and often at levels in the same range as those displayed here (e.g., Verba, Nie, and Kim 1978; Barnes, Kaase, et al. 1979a; Jennings, van Deth, et al. 1989a). Viewed in this light, more than forty years of Communist Party rule, campaigns, and exhortations have done little to differentiate China in these regards.

It is equally clear that, with other factors controlled, the presence of personal problems is no longer consequential, whereas community problems are. This is explained in part by the fact that three-fifths of those reporting personal problems felt it was up to them to

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ous behavior and civil conflict, a not uncommon historical occurrence in the Chinese countryside. Even during the economic reform era, as recently as 1993, there have been reports of peasant uprisings. See Fewsmith (1994, 89) and Liu (1996, 59–62). Compliance and resignation are other possible responses to perceived shortcomings when no relief channels seem accessible or the opportunity costs are unbearable.

[19] Personal problems mentioned ran the gamut from family conflicts to lack of sufficient income, poor housing, and inadequate supplies of agriculture aids.

[20] The list of local problems included (in this order) day-to-day supply of goods, medical care, income distribution, education, prices, public order, birth control, energy resources, transport and communications, protection of the environment, equality of the sexes, industrial development, and agricultural development.

[21] This edge was rather uniform across the township and village levels; it was not a function of a few outliers in County B.

[22] Using three county dummy variables provided little further information. Consequently, only results using the single dummy variable will be reported.

TABLE 3. Predictors of Demanding Political Activities in Four Rural Chinese Counties (Index 0–3)

Traits	(1)	(2)	(3)
Sex (0–1)	.210***	.181***	.191***
	(.043)	(.043)	(.043)
Years of schooling (0–13)	.031***	.025***	.025***
	(.007)	(.007)	(.007)
Age (18–80)	.024**	.016*	.021**
	(.008)	(.008)	(.008)
Age squared $(x\ 10^{-4})$	−2.51**	−1.87*	−2.45**
	(.87)	(.87)	(.86)
Second occupation	.241***	.266***	.261***
	(.050)	(.050)	(.049)
Personal problems (0–2)	.014	.027	−.019
	(.028)	(.028)	(.028)
Local problems (0–13)	.050***	.045***	.036***
	(.009)	(.009)	(.009)
Civic competence (0–1)	.253***	.206***	.204***
	(.042)	(.042)	(.041)
Party member (0–1)	—	.473***	.485***
		(.077)	(.076)
County B (0–1)	—	—	.315***
			(.051)
Constant	−.654	−.436	−.526
	(.173)	(.175)	(.173)
Adj. R^2	.177	.204	.228
Number of cases	1174	1159	1159

Note: Entries are unstandardized regression coefficients, with standard errors in parentheses. Data are unweighted.
*$p < .05$; **$p < .01$; ***$p < .001$.

solve their own problems, echoing results from Western countries (Heunks 1989, Sniderman and Brody 1977, Thomassen 1989).[23] From a cross-national perspective, the findings with respect to having a second occupation are unusual; if anything, people with a second job would be expected to be less participative due to the constraints of time and energy.[24]

As the second column shows, party membership has a very strong influence on participation; it raises an individual's index score, on average, one-half point. Countries vary enormously in terms of their mass membership rates,[25] but in few of them would party membership carry the duties, expectations, and clout that it does in China. Nevertheless, cross-national surveys have shown that people with party ties engage in more particularized and extended political contacting than do those without them (Zuckerman and West 1985, Brady and Kaplan 1995). China follows that pattern.

Equally important, however, adding party membership to the equation scarcely alters the existing relationships. Basically, this means that party membership is weakly related to the other traits, as the increase in the R^2 value implies.[26] Any effects of the other variables are not being disguised by party membership. It is not a proxy for other determinants, and its impact is not buried in other determinants.[27]

Finally, equation 3 shows the results when the County B dummy variable is added. Residential location obviously is correlated with a strong difference in participation. Compared with residents in the other counties, those in County B scored, on average, three-tenths of a point higher on the index, even with prominent individual-level characteristics controlled. Moreover, these effects occur without diminishing the impact of individual-level traits. The attributes that were consequential in equations 1 and 2 maintain their vitality, and the R^2 value increases noticeably. The effect of perceived local problems suffers the most when county is introduced but still remains a strong predictor. As with party membership, county of residence appears to make a separate, distinctive contribution to levels of participation.

Assuming that the apparent effects of county do not, in fact, represent some unknown individual attributes, can more be said about what particular systemic features make County B so distinctive? Caution is the watchword here; with variation limited to four counties, we are essentially confronted with a classic small N problem. The four counties vary in many respects, and any single trait that may account for County B's uniqueness is almost certainly related to other explanatory traits. Thus, the explanations offered here are plausible speculations and point toward a configuration of factors rather than any single one.

Bearing this caveat in mind, two suspects—affluence and urbanism—can be ruled out immediately. County B ranks third in income level and is not the county most proximate to a large city. Rather, cultural and structural features appear to be the distinguishing features. Informal interviews with local cadres and profiles derived from the survey data point toward the presence of social tensions as creating a climate of opinion conducive to participation. County B residents, compared with others, usually saw more social *inequality* around them, especially of an economic sort.[28] Social *unrest* also appears to be a much greater problem in this county, based on cadre as well as mass public perceptions.[29] To the degree that perceived social injustice and unrest fuels political participation, County B provides the best candidate of the four being studied.

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[23] A finer delineation suggests that certain types of personal problems do lead to political action. The main point of the present analysis, however, is to distinguish between the effect of problems at the community and personal level.

[24] Additional analysis revealed that holding a second job had a more positive effect on the participation level of women than of men.

[25] A five-nation study in the 1970s reported party membership ranging from a high of 28% in Austria to lows of 4% in the Japan and the United States (Verba, Nie, and Kim 1978). Another 1970s study of eight Western nations found a comparable range, with the average being around 13% (Barnes, Kaase et al. 1979b).

[26] The strongest correlation is with sex, $r = .19$. Other correlations are: competence, .17; age, .14; education, .10; local problems, .08; second job, .00; and personal problems, −.03.

[27] Another way to demonstrate this is to rerun the original equation (column 1) after deleting the party members from the base. The differences are very slight: Without party members, the adjusted R^2 drops to .17, the coefficient for sex decreases slightly (but is still significant at .001), and the coefficients for age and age squared also decrease in significance from .01 to around .05. Other predictors are scarcely affected. By the same token, a separate equation estimated only for party members produces quite different coefficients for several of the predictors—most notably sex (.546), education (−.004), local problems (.103), and competence (.047). Only one of the eight predictors is significant at the .05 level, due in part to the small subset of party members ($N = 84$).

[28] Respondents there more often opined that some people were receiving more than their just desserts (70% versus 61% for the other three counties combined); accorded the "newly rich" in Chinese society (the 10,000-yuan households) the lowest score on the 0–100°

More explicitly political factors also seem to be at work. Research team leaders came away from the four counties with the impression that the Communist Party is better organized in County B and is supportive of grass-roots participation.[30] Indeed, systematic interviews with samples of government, party, and enterprise leaders revealed that County B elites were among the least likely to agree that "only those with the relevant expertise or competence have the right to have their say on an issue" (20% versus 40% for the other three counties combined); that "popular participation is unnecessary so long as the leaders are capable and enjoy the people's confidence" (12% versus 24%); and that "only some of the simpler issues can be put forward for considerations by the general public" (31% versus 39%). Congruent with this elite perspective is the fact that County B citizens compared to those in the other counties combined, more often claimed to be *very* interested in village (31% versus 18%) and township (34% versus 22%) elections. In sum, certain structural properties and civic attitudes also create a favorable environment for participation in County B.

Leaving aside the question of what sets County B apart from the others, the more important point is that the odds of a citizen participating in a more vigorous fashion depend upon local features over and above personal traits. As seasoned observers have long recognized, contextual effects and local variations abound in China. What remains is to heed the calls (e.g., Perry 1994) for more systematic comparisons across time and space in the study of Chinese politics in the wake of new research opportunities.

CONCLUSION

Plotting the trajectory of China's movement toward a more participatory political system is a foolhardy enterprise. Nevertheless, the results from this analysis of local participation are instructive on several counts. To the degree that democratization is occurring in China, the signs are most evident at the local level. Thus, the patterns of participation observed there may be a harbinger for eventual changes at higher levels as well as a foreshadowing of further changes at the local level.

A legitimate question is whether the types of political activity portrayed here constitute meaningful participation. A frequent characterization of local participation in China stresses its essentially atomistic, petitioning, selective goods-producing nature. To some degree the thrust of the present findings points in that direction. Contacting, voicing, and even cooperative actions more often than not had the ring of behavior taken by an individual or small group to generate selective benefits. This is a rational strategy in a relatively closed system and parallels a depiction of political life in the former Soviet Union (DiFranceisco and Gitelman 1984).

Nevertheless, distinct signs of activities in pursuit of collective goods also emerged. A visible minority of the sample specifically acknowledged engaging in collective action as such by reporting that they had banded together with others to address a common problem. Perhaps more significantly, individuals engaged in the other two participation modes—voicing opinion and contacting delegates—placed substantial emphasis on addressing such communal topics as infrastructure needs, certain agriculture policies, and economic enterprise concerns. Even if the participants attacked these problems as solo actors, the goods involved were often collective in nature. Somewhat surprisingly, a relatively large amount of activity occurred in the electoral domain, where collective as well as selective benefits could be gained.

Local participants also emerged as strategic actors. The particular mode of participation seemed to be a combined function of the issue at hand, the opportunity structure, and the particular resources of the individual. Thus, the frequency of a given mode was related in part to the ease and multiplicity of access. What is more significant, those using a given mode tended to associate it with particular kinds of issues, rather than squander it on issues for which it would be less effective (see Falkenheim 1987, 3–6). For example, those using delegate contacts as a participation mode stressed personal economic problems much more than infrastructure and redistribution problems, presumably because they saw delegates as being in a better position to affect the former than the latter. By contrast, those engaging in the collective action mode downplayed personal economic problems and emphasized infrastructure and redistribution matters, again reflecting an effort to match the problem with the more likely solution source.

This view of a more proactive, collective, and strategic version of local participation is sympathetic with a recent line of theorizing and intensive field research that posits a "rightful resistance" concept of participation at the village level (Li and O'Brien 1996, O'Brien 1996). While acknowledging that popular participation still occurs largely as a response to policies from above, this approach emphasizes economic and political reform as having facilitated the ability of villagers to use official policies and values to confront leaders when grievances develop. Significant challenges of this sort

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feeling thermometer (55.2 versus 63.9); and more often rated income distribution as a problem in their local area (40% versus 25%).

[29] For example, far more County B residents rated public order as a local problem (67% versus 22% for the other three counties combined); gave more priority to the value of maintaining order as compared with controlling prices (ratio of 2.5 versus 1.6); and more often viewed local cadre corruption as being quite serious (56% versus 48%).

[30] There is absolutely no indication that County B officials engineered the higher rates of reported participation by circulating their desires to have a local "good showing." Indeed, County B had inferior marks on many other characteristics, as just noted with respect to higher complaints about corruption and public security, and including satisfaction with living in the county.

typically include more than a single individual, often rest on either explicit or implicit claims to rights and contractual obligations, and involve collective benefits.

Even if one were to cast the participatory acts treated in this paper as proto or pseudo forms of participation, the degree to which several of the correlates of participation resemble those found in more developed participatory systems is remarkable. Indeed, an innocent observer looking at these results might easily locate them in a Western democracy. Particularly striking is the influence of education and sex as determinants of participation. Residues of an historically strong patriarchal culture continue to influence mightily the political activity of ordinary citizens. By the same token, the privileges and skills associated with education are fully as operative, if not more so, as in Western societies. Absent voluntary associations and independent interest groups that might serve as compensatory mechanisms, resources rooted in individual-level traits come to the fore.

At the same time, unique aspects emerge. Being a party member privileges the countryside resident. The sheer size of the party, coupled with its dominant force in political life, makes these rates far more consequential than would comparable participation rates in countries with smaller membership bases and less monopolistic roles in the party and governing systems. Whether the advantage of party membership is greater or lesser than before the decade of reform is problematic, but the development of other political resource bases—as in township and village enterprises and networks of specialized households—as well as various political reforms suggest that the advantage of party membership may have declined.

Another unique aspect is the role of occupational standing. Even though occupational status affects participation in other systems, the material and symbolic significance of having a second job is perhaps unique to China and other systems experiencing a rapid shift to market capitalism. Having two occupations appears to widen the scope of claims and grievances and expands the individual's networks. As the market economy matures, the second occupation classification may become even more potent.

Although modest political reform in the countryside seems reasonably well established as of this writing, its fragile and variable nature warrants caution. Strong action by organs of the central government, to say nothing of machinations by local cadres, can threaten the slow movement toward a more open, participatory system. Reforms also can be threatened by economic downturns and what is often described as rampant corruption. Variability, which is actually a staple of local governments and economies in China, is vividly reflected in the higher participation profile of one of the four counties included in this study. Over and above individual-level traits stand some contextual properties that facilitate participation in this county. Just as the fruits of economic reform continue to be distributed with varying success and rapidity throughout the country, so, too, will those of political reform.

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The Public Consequences of Private Inequality: Family Life and Citizen Participation

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This study uses regression analysis of data from a telephone survey of 380 married couples to subject to rare empirical test the contention that, because women are unequal at home, they cannot be equal in the polity. The argument is often made that wives' disadvantage in comparison to their husbands with respect to control over family income, availability of free time, power over decisions, or mutual respect dampens their ability to participate fully in politics. In fact, domestic inequalities do have implications for political activity, but these effects differ from what is usually posited by being stronger for husbands than for wives. For husbands, control over major financial decisions and autonomy in using small amounts of time enhance their ability to participate in politics beyond what would be expected on the basis of their other characteristics. In short, being boss at home is politically empowering to husbands.

The family is society at its most private; the state is society at its most public. Yet, they are connected in multiple ways. Various lines of inquiry in political science have probed the consequences for the politics of community and nation of what happens inside the home. For example, on the presumption that the child is parent to the adult, studies of political socialization focus on the transmission of political ideals and habits across the generations. Observers of politics in places as disparate as Nazi Germany and postwar Japan have remarked upon the implications for the character of the regime of the nature of authority relations within the family. More recently, feminist theorists have focused on husbands and wives, arguing that the inequalities in domestic arrangements prevent women from becoming full citizens within the polity. In this article we use the results of a survey of married couples to subject this contention to the light of data, inquiring as to the effect of inequalities within the household on women's and men's participation in politics. In the process we illumine further how adult experiences outside politics can have implications for the political comportment of citizens.

FEMINIST THEORY, THE FAMILY, AND CITIZEN PARTICIPATION

One theme of recent feminist theory is that, until women are equal at home, they cannot be equal in the polity. In her pathbreaking study, Susan Okin (1989, 22) argues that, although patriarchal domestic relationships have consequences for women's ability to participate fully as citizens, theorists of justice have either neglected the family or exempted the family from the principles by which the polity must be evaluated. "In a just society, the structures and practices of families must give women the same opportunities as men to develop their capacities, to participate in political power and influence social choices, and to be economically secure."

An especially important variant of this line of reasoning emphasizes the way that an unequal division of labor at home—with women assuming a disproportionate share of the domestic responsibilities—deprives women of the essential political resource of time and, thus, compromises their ability to be active in politics. Anne Phillips (1991, 96–7) offers a particularly articulate statement of this point of view:

> Women are prevented from participating in public life because of the way their private lives are run. The division of labor between women and men constitutes for most women a double burden of work.... The mere pressures of time will keep most women out of any of the processes of decision-making ... the way our private lives are organized promotes male involvement and reduces female participation. Who collects the children and who makes the tea is [sic] a vital political concern.... Whether at the simplest level of having no free time, or as a more complex consequence of always being told what to do, women's experiences in the home continually undercut the possibilities for democracy.

For the student of political participation, this theoretical formulation presents several difficulties. First, theorists who characterize the family as an incubator of citizen inequality draw out the implications for women's participation in politics but ignore the effect of domestic inequalities on *men's* participation. In this article we allow for the possibility that domestic hierarchy of various kinds may affect men's as well as women's comportment in politics.

Second, this formulation offers only a limited view of the multiple possibilities for inequality at home and is insufficiently specific as to how each aspect may operate to influence the political activity of the members of a couple. In our analysis we broaden the formulation

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and give it greater clarity, testing the following possibilities.

Resources: Resource constraints with respect to either time or money may depress participation. That is, differing positions in the labor market or differing responsibilities at home may leave either spouse in the position of having a greater stockpile of leisure time or personal income, resources that may facilitate political activity.

Family Social Structure: Implicit within the distribution of tasks or authority in the household are lessons that might have implications for the predisposition to take part in politics. Role differentiation and patterns of authority within the family may affect political activity in either or both of two ways:

A. Those who do not share fully in authority over family decisions or who find that they do not enjoy equal respect may extrapolate from their experiences and feel less efficacious in the political realm.
B. Those who assume the burden for running the household and rearing children may receive the implicit message that their energies are to be focused inward on the home rather than on the public world outside.

Our analysis leaves open the possibility that, on any particular dimension, husbands and wives may be equal or unequal and, if the latter, either spouse may be advantaged. Furthermore, we do not assume that the implications for political activity of inequality with respect to any of these dimensions must be the same for wives and husbands.

Third, in emphasizing the resource constraints imposed on women by the traditional family, feminist theory is silent as to the potential effects on political activity of ideology, that is, the role played by *beliefs* about appropriate gender roles. There has long been a tension within the social sciences—dating back to differences between the successors to Marx and Weber—over whether social action springs from material conditions or whether values and beliefs have an independent influence on behavior. Recent scholarship argues that ideas about gender roles operate on their own to shape family life, above and beyond the influences of relative resources, and affect the behavior of both men and women (e.g., Brines 1994, Thompson and Walker 1989, West and Zimmerman 1987). Working from a similar perspective, political scientists (Sapiro 1983, Tolleson-Rinehart 1992) argue that ideas about women's proper roles affect women's political behavior.

A final difficulty is that, by the theorists' own admission, the empirical evidence on which this theory is predicated is slender. Phillips—who asserts (1991, 97) "I consider the argument won almost as soon as it is stated"—seems to find the connection between inequality at home and inequality in politics to be so obvious that the absence of empirical confirmation is hardly a liability. To us, however, the lack of evidence is problematic.

INEQUALITY AT HOME AND INEQUALITY IN POLITICS: EVIDENCE FROM OTHER STUDIES

Previous research does provide empirical evidence about parts of this puzzle taken separately. Scholars of the family provide ample data to confirm that husbands and wives are not equal—in their power, in their access to and control over such vital resources as time and money, or in their household responsibilities (see, e.g., Blood and Wolfe 1968; Blumstein and Schwartz 1983; Brines 1994; Epstein 1988, chapter 9; Goode 1971; Huston and Geis 1993; Lennon and Rosenfield 1994; Pahl 1989; Rubin 1992; Starrels 1994; Thompson and Walker 1989; Vogler and Pahl 1994). In particular, many studies confirm that married couples do not share household and child-rearing chores equally, even when both partners have full-time jobs (e.g., Bird, Bird, and Scruggs 1984; Blumberg and Coleman 1989; Browning 1992; Douthitt 1989; Hochschild 1989; Oakley 1974; and South and Spitze 1994).

In addition, there is extensive information about gender roles in politics. Men and women differ substantially in terms of the power they exercise as political elites: The higher up the political ladder, the more severe is the underrepresentation of women. Among ordinary citizens, however, there is more similarity than difference between women and men in terms of the amount and kind of their political activity.[1] To the extent that there are differences, men are somewhat more active than women. The size of the gender gap, however, does not vary systematically with the difficulty of the act, nor do active women specialize—as feminist theory would predict them to do—in voluntary organizational, nonelectoral, or informal community activity. Interestingly, the greatest gender disparity appears in connection with making financial contributions to electoral campaigns or other political causes. Compared with women, men not only are more likely to give money but also write larger checks when they contribute. In addition, contrary to expectation, women do not specialize in either organizational or informal community activity. In terms of the origins of participation, the resources that facilitate political activity—including education, income, and civic skills—operate similarly for men and women, but women have fewer of them. A significant portion of the gender gap in levels of overall participation and nearly all the gender gap in levels of political contributions can be explained by the cumulative effects of the masculine advantage with respect to the resources that foster participation, especially money.[2]

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[1] This paragraph constitutes a very brief summary of an analysis conducted earlier. For much more detailed discussion and extensive bibliographical references, see Schlozman, Burns, and Verba 1994. Among the many additional relevant works, see, for example, Beckwith 1986, Sapiro 1983, Tolleson-Rinehart 1992, and Welch 1977.

[2] Although married women and men are, on average, quite active in politics—a reflection of their relative socioeconomic advantage—marriage itself does not enhance political activity for either sex. Stoker and Jennings (1995) examine marriage in a dynamic setting

INSIDE THE HOUSEHOLD: THE MULTIPLE DIMENSIONS OF INEQUALITY

The literature yields numerous insights about gender inequalities in the family and in politics. This article attempts to provide the missing empirical link between the two domains by assessing the effect of domestic inequalities on the political participation of married men and women. Our understanding of political participation is quite broad, encompassing not only voting and making contributions of time or money to campaigns but also nonelectoral forms of participation that are sometimes, though not always correctly, considered to be more congenial to women: contacting public officials, attending protests, participating in informal community efforts, serving on local governing boards, and becoming involved in political organizations.

We also construe inequalities between wife and husband quite broadly. Drawing on Weber's (1958) insight that societies are stratified on multiple bases, we consider several kinds of hierarchy *within* the household. That is, we conceptualize the family as a microeconomy, micropolity, and microsociety and consider asymmetries along various dimensions: the relative position of wives and husbands in their resources, their voice in family decisions, and the respect they accord each other. We are concerned not simply with differences on any of these dimensions but with their hierarchical nature and with the effect of relative advantage of the husband or wife on the participation of either member of the couple.

Before proceeding to the dimensions of inequality, it is important to differentiate what we are attempting from the well-known SES model of participation. According to that model, those who are well endowed with socioeconomic resources are more likely to be active in politics.[3] Our enterprise is to apply this logic to the private domain of the family, not to the world outside the home. We want to ascertain for each marriage partner whether relative advantage within the family—that is, privilege in terms of the amount of free time or money enjoyed, autonomy in decision making, or respect—facilitates political activity.

Our first concern is with resources. It is well known that family income is a crucial resource for participation in politics, especially for the ability to make political contributions. Yet, the SES model treats family income as equally enabling to each marriage partner regardless of whose wages, inheritance, or lottery ticket brought it into the household. Because we are looking inside the household, we ask whether relative contributions to total family income have similar implications for the ability of husband and wife to take part. Hence, we include measures gauging the relative economic wherewithal of the husband and wife, that is, who brings money into the household.[4]

Given the emphasis in the literature upon the way differential responsibilities for household chores constrain the time women have available for politics, we are particularly interested in probing the role of time. Therefore, we include in our analysis measures of the relative amount of time each partner contributes to household management.

What is known about time as a resource for political activity is somewhat surprising. The availability of leisure time, in general, plays an unexpectedly weak role in fostering political activity. How much leisure time people command, that is, how many hours they have left after accounting for time devoted to paid work, household tasks, and school, has much less effect on their political activity than does, say, their level of education or interest in politics (Brady, Verba, and Schlozman 1995). Nevertheless, given the emphasis placed upon time constraints in explanations of gender differences in political activity, it is important to look closely at these constraints in the domestic context.

We also consider several aspects of inequality related to family social structure. Closely related to having money or time is decision-making autonomy—the power to make independent decisions—with respect to the allocation of whatever money or time is available to the family. What may really count is not the amount of resources available but the *control* over these resources. Furthermore, individuals may generalize from their experiences with decision making in the family to their roles in the polity. Exercising power in the family, either as an equal partner or as the dominant partner in decisions, may enhance feelings of political competence, an expectation supported by work on the family as an agent of socialization (Almond and Verba 1963, 324–35; Sapiro 1983). Thus, control over money or time may have an effect on both actual political resources and the motivation to use them in politics.

We are also concerned with respect within the family. There may be stratification at home, just as in the world outside, in terms of social respect. Couples differ in the level of respect accorded each partner and in the extent to which that respect is mutual or asymmetrical. Enjoying relative respect may have the consequence of enhancing the confidence with which a family member confronts the wider political world.

We are unsure what to expect regarding the effects of a traditional division of labor upon the ability to take part in politics, apart from the consequences for access to such resources as time and money. A disproportionate share of the responsibility for vacuuming and grocery shopping may negatively affect participation, perhaps by constricting the opportunities to develop the capacities critical for political activity or by rein-

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and demonstrate that the effect of marriage depends to a large degree upon the prior participatory histories of the partners.

[3] The relationship between SES and political participation is well documented. See, for example, Bennett and Bennett 1986, Conway 1991, Milbrath and Goel 1977, and Verba and Nie 1972. For extended discussion of the SES model and a much more elaborated model based on resources, see Verba, Schlozman, and Brady 1995, especially chapters 9, 12, and 15.

[4] Blumberg and Coleman (1989) develop a theory of gender power within the family that stresses the importance of who brings money into the home, rather than the job per se. They do not, however, consider the implications of economic control for political participation.

forcing the stereotype that those who mind the hearth should not venture into the public world of politics. Alternatively, the critical factor may be time: So long as the partners are equally busy, it may not matter who does what. That is, separate but equal may be genuinely equal when it comes to the division of labor at home.

In addition to assessing the consequences for political activity of inequalities in class, status, and power at home, this inquiry addresses another issue: the role of beliefs about appropriate gender roles in enhancing political activity. As we mentioned, recent scholarship on the family has returned to socialization theory to ask whether ideas about gender roles shape marital interactions above and beyond the influences of relative resources. We are interested in whether views about gender roles have consequences for political activity, above and beyond their effect on the division of labor within the household.

THE DATA

The data we report derive from the third wave of a major study of citizen participation. The first wave involved a telephone screening interview with 15,000 randomly selected respondents from across the country. A second wave consisted of face-to-face interviews with 2,500 respondents selected from those 15,000 in a manner to provide larger numbers of political activists as well as members of minority groups. For the third wave we conducted telephone interviews with 609 of the respondents from the second wave, using items about social characteristics and voluntary activity from the initial questionnaires as well as new items about family characteristics. (See Appendix A for a fuller description of the samples for the first and second waves.) Of this group, 380 were married at the time of the third interview.[5] For married respondents we also conducted separate interviews—using special techniques to ensure that the members of the couple could not monitor each other's answers—with their spouses.[6] These data provide basic information on the members of the couples sample who were entering our study for the first time and up-to-date information on the people interviewed in the first and second waves. In addition, we have independent reports from each spouse about family and household matters. Thus, we are able to combine information from wife and husband to typify the family. We can also consider inconsistencies in reports from partners and incorporate any discrepancies into our characterization of the family.

THE MEASURES OF FAMILY EQUALITY: DESCRIPTIVE DATA

Because issues of family equality are not ordinary fare for political scientists, we consider it important to describe in detail the variables used to measure it. For each of the several dimensions on which there might be family hierarchy, we present the views of both husband and wife. These data can be presented in several ways: as a comparison of the aggregate characteristics of husband and wife, as a measurement of the level of agreement within couples,[7] and as typifying the joint characteristics of both spouses within a couple. We shall use one or more of these modes of data presentation where appropriate.

Resources: Money and Time

Table 1 presents data about two resources essential for running a household: money and time. The top row shows the average estimates reported by wives and by husbands of the proportion of family income brought in by the wife by virtue of her wages and other sources of income (such as child support, rental income, or dividends) attributable to her.[8] There is, in the aggregate, remarkable agreement between women and men that the wife brings in about one-third of the household income, a figure which accords with that found in other surveys (Thompson and Walker 1989, 850). That wives contribute less than an equal share to the family exchequer is related, presumably, to the fact that, compared to men, women are less likely to work for pay and earn less when they work full time. In short, women seem to be at a disadvantage when it comes to command of family financial resources. The person who brings income into the family may be able to exercise greater control over its uses for family or individual purposes (an issue to which we shall return shortly).

In addition to the averages for wives taken together and husbands taken together, Table 1 presents data about whether marriage partners agree about these matters. Members of married couples seem to be in rough, though not perfect, agreement as to the proportion of the family exchequer contributed by the wife.

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[5] It is important to note that the respondent's marital status at the time of the second wave was not a criterion in interviewing spouses. Thus, we did not specifically select couples who were married in both waves, which would have overrepresented those with marriages of longer duration.

We considered interviewing the domestic partners of unmarried, heterosexual, and homosexual respondents, but there were simply too few respondents in these categories to pursue this approach. A study that seeks comparisons of married and unmarried couples would need to follow a strategy analogous to ours, a large initial screener followed by oversampling of respondents in "living-together" arrangements, and probably would require an even larger screener sample to begin with.

[6] We mailed respondents a series of cards, analogous to those used to inquire about family income in the personal interview. These cards contained the alternative answers for many items, especially sensitive ones. When answering a question, the respondent was directed to say the letter corresponding to the response category rather than to express the response in words. In this way, someone else in the room would have difficulty knowing what the respondent was saying to the interviewer over the phone. Thus, we added to the existing advantages of surveying couples by telephone: "By telephone, others present in the room cannot hear the questions and may have little information upon which to guess the meaning of the answers. Telephone interviews may feel more private, since third parties only hear one side" (Aquilino 1993, 375).

[7] Where there are discrepancies between the reports of each spouse, we make no assumption as to which partner should be trusted.

[8] Question wording for these and all items can be found in Appendix B.

TABLE 1. Resource Equality: Proportion of Total Household Income and Housework Attributable to Wife

	Wives' Report	Husbands' Report	Average Difference Between Spouses[a]	Correlation between Spouses
Income (percentage of family income contributed by wife)	33% (26)	31% (27)	18% (21)	.44
Housework (percentage of total hours of housework wife does)	69% (18)	62% (18)	15% (13)	.45
Number of cases				
Income	367	344		
Housework	372	348		

Note: Standard deviations in parentheses.
[a] Average of the absolute values of the difference in the responses of the members of a couple.

On average, spouses differ by 18% in their estimate of the proportion of family income attributable to the wife.[9] The correlation between their estimates is .44, which is significant at the .001 level.

Table 1 also contains data on the wife's share of total hours devoted by both members of the couple to necessary household work. Respondents were asked to estimate for themselves and their spouse how much time each spent on a variety of activities in an average day: gainful employment, including commuting and work taken home; necessary household tasks (with childcare as a subset); taking courses for credit (and studying for them); and sleeping. As before, we present figures for wives taken together and husbands taken together. There is relatively little difference between the average estimate by husbands and by wives of the proportion contributed by the wife to the total hours devoted to housework. Interestingly, the average estimates for contributions of household time and money are mirror images: Wives contribute about one-third of the income and just under two-thirds of the housework time.[10] In addition, there is significantly more variability in the assessments of the proportion of household income contributed by the wife than in the assessments of the proportion of household labor she contributes.[11] Table 1 also presents information about the level of agreement within married couples about how much of the total housework the wife does. Once again, the data show rough but not perfect agreement between spouses.

It must be noted, however, that despite the fact that they contribute more time to household chores than do men, women, on average, do not appear to have less free time than men. Among married respondents—the subjects of this study—men and women reported an identical amount of free time per day: six hours.[12] Among those married respondents with full-time jobs and children at home, women have, on average, less free time (3.05 hours) than do similarly situated men (3.45 hours). Yet, the difference is smaller than might have been expected on the basis of the division of labor at home because men with full-time jobs and children at home tend to put in longer hours at work. In sum, men and women differ little in the amount of time they have available after they have honored their commitments to job, household, and school.[13]

We can amplify these findings about who does what at home. For several common household chores—cleaning, grocery shopping, paying bills, taking care of the car, and caring for the children—we asked each partner whether s/he did all, most, some, little, or none of that task. Table 2 shows the distribution of the aggregate responses for wives and for husbands. Overall, a similar division of labor is reported, with wives doing more when it comes to cleaning, shopping, caring for the children, and, to a lesser extent, paying bills, and with men taking primary responsibility for the car. When we consider couples and compare spouses, we find some tendency toward claiming credit: Ordinarily, each partner makes a more generous assessment of his or her own effort than, by inference, does the spouse. Yet, the proportion of couples in which the spouses give contradictory answers—each claiming to do all or most of a particular chore—is relatively low. For each of the tasks, except for repairing the car, couples give contradictory reports less than one-tenth of the time. The fewest contradictory responses are found in relation to traditional female chores: taking care of the

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[9] This figure is an average of absolute values. Thus, a couple in which the wife's estimate of her contribution is, say, 15% *higher* than the husband's estimate of her contribution is not balanced by a couple in which her estimate of her contribution is 15% *lower* than his estimate of her contribution.

[10] Although it would have been preferable to base our data on time diaries, we are encouraged that our findings about the proportion of time given to household tasks are consistent with those of other studies. For example, Schor (1992) estimates that women contributed 72% of the household time in 1967 and 63% in 1987. Using a range of methods for ascertaining relative contributions of household labor, numerous studies (Huston and Geis 1993, 94; Lennon and Rosenfield 1994; South and Spitze 1994) arrive at similar results.

[11] For the combined estimates of household time and money used below (see appendices B and C for explanation of the construction of these variables), women contribute 32% of the household money, with a standard deviation of 25. In contrast, women contribute 61% of the household labor, with the smaller standard deviation of 16.

[12] Gender parity in access to free time also has been noted in the family studies literature. See Thompson and Walker (1989, 850) and Zick and McCullough (1991, 471).

[13] Secondary analysis of data using similar questions from a study conducted four decades earlier show substantial change in some respects, not so much in others. In 1955, women reported bringing in 13% of the family income compared with 33% in the recent study, but their contribution to household work has not diminished proportionately. In the earlier study, they reported doing 69%, more recently 69% (Axelrod and Blood [1955] 1974).

TABLE 2. Division of Household Chores

	All	Most	Some	Little	None		N	Contradictory Responses[a]
Housecleaning								
Wives say they do	41%	34%	17%	3%	5%	= 100	374	7%
Husbands say they do	1	10	46	31	12	= 100	349	
Grocery shopping								
Wives say they do	47	34	14	2	4	= 100	374	9%
Husbands say they do	9	13	38	25	15	= 100	349	
Paying bills								
Wives say they do	48	17	13	12	11	= 101	374	9%
Husbands say they do	21	20	14	20	25	= 100	348	
Repairs/car								
Wives say they do	7	13	29	27	24	= 100	374	17%
Husbands say they do	69	24	5	1	1	= 100	349	
Taking care of the children								
Wives say they do	11	63	22	1	4	= 101	230	5%
Husbands say they do	[b]	7	75	10	9	= 101	224	

[a] Both members of a couple claim to do all or most of the chore.
[b] Less than 1%.

children and cleaning the house. Most wives—and few husbands—claim to do all or most of these tasks.

We can also embellish the data in Table 1 on the relative economic position of the spouses. As a corollary to the data about relative contributions to family income, we have included in Table 3 information about the occupational levels of respondents. The data in the upper portion of the table show the distribution on a scale measuring respondents' assessments of the amount of education and on-the-job training required for their jobs. Considering just those who are currently in the work force, wives are less likely than husbands to be in high-level occupations. In the lower portion of the table we present data, for those in the work force, comparing the job levels of members of couples (the husband's job level minus the wife's). The preponderance of positive numbers shows the expected result: husbands are likely to have higher status occupations than their wives.

TABLE 3. Job Levels of Husbands and Wives

A. Education and on-the-job training required

		Work Force Members[a]	
		Wives	Husbands
Least education and on-the-job training	1	15%	9%
	2	32	14
	3	13	34
	4	31	17
Most education and on-the-job training	5	9	26
		100%	100%
N		313	333

B. Comparative job levels of members of couple:
Husband's job level minus wife's job level

−4	−3	−2	−1	0	+1	+2	+3	+4		N
[b]	[b]	6	18	29	32	7	6	3	= 101%	274

[a] Eighteen percent of the wives and 13% of the husbands have not had a job for as long as one year.
[b] Less than 1%.

Control over Money and Time

Beyond the amount that each spouse contributes to the family is the issue of who controls the use of the family stockpile of time or money. We asked both a question about how much responsibility each takes for "bigger financial decisions"[14] and a series of questions about whether they can make a decision on their own to give "a few hours" to a charity drive, political cause, or something they enjoy doing or whether their spouse would have some say in a decision to use time in these ways. Table 4 presents the responses. Financial decision making is a domain in which husbands report, on average, that they exercise greater control than is reported by wives. Nearly three-quarters of the husbands indicated taking all or most of the responsibility for big financial decisions, whereas only one-quarter of the wives made such a claim.[15] The agreement between spouses is fairly high; in only 15% of the couples do both spouses claim to make all or most of the big financial decisions.

With respect to control over time, we were surprised to find a very different pattern. Compared with husbands, wives were more likely to report being able to give time to voluntary activities—charitable, political, or social—without consultation. We guessed that this unexpected finding might reflect the fact that those who are not in the work force, a group that is disproportionately female, would exercise particular control

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[14] Vogler and Pahl (1994, 273–4) use a similar measure of control over large household decisions as an indicator of "strategic control over household finances."

[15] Reanalysis of the earlier survey shows that wives were even less likely to make financial decisions forty years ago. In 1955 nearly two-thirds of wives reported having little or no responsibility for major financial matters, in contrast to only 9% who made this assessment in the recent survey (Axelrod and Blood [1955] 1974).

TABLE 4. Control over Money and Time

A. Responsibility for the Larger Financial Decisions, by Sex

	All	Most	Some	Little	None		N	Contradictory Responses[a]
Wives say they have	7%	19	65	7	2	= 100%	373	15%
Husbands say they have	17%	55	26	1	[b]	= 99%	348	

B. Comparative Control of Free Time of Husbands and Wives[c]

	Combined Report of Wife and Husband
Husband has more freedom	7%
Members of couple have equal freedom	62
Wife has more freedom	31
	100%
N	329

Number of times out of three respondent reports that spouse would have some say in respondent's decision about how to spend a few hours

	Wives' Report	Husbands' Report
0	62%	42%
1	23	19
2	10	20
3	6	19
	101%	100%
N	368	340

[a] Both members of couple say they have all or most of the responsibility.

[b] Less than 1%.

[c] Based on answers to the following question: "Would your spouse have some say about how you spend a few hours (on a charity drive, on something you enjoy, on a political campaign or cause)?"

over their time. When we looked at the data, however, this hypothesis was discredited: Women who work full time are as likely as women at home to make autonomous decisions about the use of small amounts of time.

Respect

Measuring the relative respect of husband and wife for each other is extremely difficult. In a series of questions adapted from the General Social Survey, we asked respondents to choose, in order, three people (e.g., their husband or wife, a friend, a member of the clergy) whose judgment they really trust and with whom they might be likely to discuss important matters. Substantial majorities—73% of the wives and 68% of the husbands—listed their spouse first. Our measure of mutuality of respect involves comparing the responses of husbands and wives to ascertain whether they rank each other at the same place on the list or whether one partner places the other higher on the list than vice versa. This measure indicates considerable symmetry in the rankings of spouses. Sixty percent of the couples put each other at the same point on this list.[16] In 22% of couples, she ranks him higher than he ranks her; in 18% of couples the situation is reversed. In short, there is a good deal of mutual respect in these couples. Asymmetries—one spouse accords the other more respect than she or he receives in return—are relatively rare and relatively balanced. There is a tilt in favor of the husband, but it is quite small.

Beliefs about Gender Equality

Finally, we consider beliefs about equality between the sexes. Reflecting our concern with the implications of domestic inequalities for political participation, we asked several questions about attitudes toward equality at home between husband and wife: whether a woman should quit her job if it conflicts with her roles as wife and mother; whether household chores should be shared equally between spouses; and whether it is best if just the husband is in charge of family decisions. We deliberately chose these items because they ask about gender roles at home and are devoid of explicit political content.

Table 5, which presents the responses, shows that the three items elicited, on average, surprisingly similar expressions of support for gender equality at home from wives and husbands, although wives seem to have somewhat stronger views on these issues.[17] Fifty-seven percent of wives and 55% of husbands reject the idea that a woman should quit her job if it interferes with her roles at home; 90% of husbands and of wives agree

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[16] We are concerned with the relative respect accorded by members of the couple. Hence, low but equal levels of respect count as equality in this measure: We define the circumstance in which neither partner confides in the other (the case for three couples in our sample) as conferring no relative disadvantage. The overwhelming share of the equal-respect couples (88%) are at the highest level of mutual respect.

[17] Huston and Geis (1993, 93) find this same similarity between husbands and wives. Data from the Detroit Area Study reported in Thornton and Freedman (1979) indicate that the views of women have moved in a more egalitarian direction over the past three decades.

TABLE 5. Beliefs about Equality in the Family

	Primacy of Domestic Role for Wife[a]		Belief in Gender Equality in Sharing Household Chores[b]		Primacy of Husband in Decision Making[c]	
	Wives	Husbands	Wives	Husbands	Wives	Husbands
Strongly agree	15%	11%	34%	24%	2%	2%
Agree somewhat	28	34	56	66	5	5
Disagree somewhat	45	45	10	10	35	67
Strongly disagree	12	10	[d]	[d]	58	26
	100%	100%	100%	100%	100%	100%
Contradictory responses[e]	37%		19%		12%	
N	364	341	375	349	373	348

[a] "A wife should give up her job whenever it interferes with her role as a wife and mother."
[b] "Husbands and wives should share household chores equally."
[c] "It's best if just the husband is in charge of making family decisions."
[d] Less than 1%.
[e] One member of the couple agrees strongly or somewhat; the other member of the couple disagrees strongly or somewhat.

with the statement that household chores should be shared equally; and 93% of wives and of husbands disagree with the idea that men should make the family decisions. Once again, marriage partners tend to, but do not inevitably, agree on these matters. A substantial minority of couples—37%—find themselves on the opposite side of the fence, with one spouse agreeing and the other disagreeing when it comes to whether a wife should quit her job. For the other two items, the proportion of couples who disagree is much lower. And very few couples—less than 1% in each case—have opposing opinions that are strongly held. Although the data suggest a widespread belief in equality at home, we must remember the lessons of tables 2, 3, and 4, which showed considerable stereotyping in the actual division of labor. For instance, though more than nine out of ten wives and husbands disagree with the idea that just the husband should be in charge of making family decisions (Table 5), 72% of husbands but only 26% of wives report that they have responsibility for major financial decisions (Table 4). These findings are consistent with longitudinal data showing that, when it comes to gender equality in the division of household chores and responsibilities, beliefs appear to have outrun practice (Scanzoni and Fox 1980)

Equality at Home: Summary

The data in tables 1–5 do not lend themselves to simple summary. There is considerable evidence of traditional arrangements. Husbands bring in a disproportionate share of the family income and are more likely to exercise power in managing it. In addition, there is stereotyping in who is responsible for which family chores, with wives assuming a greater share of the household work—relatively egalitarian beliefs of both husbands and wives to the contrary. Nevertheless, although women spend more time on housework, in the aggregate they do not lack free time. Moreover, wives seem to exercise greater autonomy than husbands in making decisions about the use of small amounts of time. Finally, husbands and wives do not differ in the way they rank the other in the list of those they really trust.

EQUALITY AT HOME AND ACTIVITY IN POLITICS

Our goal is to assess the effect of inequalities at home on the political activity of marriage partners. To do so, we use separate ordinary least-squares (OLS) regressions to ascertain the factors that predict political participation for wives and husbands. This will permit us to understand the extent to which, all else being equal, various family patterns affect the participation of each partner. In interpreting these results it is important to distinguish between, on the one hand, the distribution of a particular factor between husbands and wives and, on the other, the size and direction of its effect on participation for either set of marriage partners. For example, neither husbands nor wives are, in the aggregate, advantaged in terms of the relative respect they receive at home, but enjoying respect at home might give a boost to the activity of either wives or husbands, or both, or neither.

The dependent variable in this analysis is an overall summary of an individual's political activity, an additive scale based on eight activities: voting, working in campaigns, making campaign contributions, contacting public officials, taking part in protests, working informally with others to solve community problems, belonging to local governing boards, and affiliating with political organizations. (For scale construction and its characteristics, see Appendix B.) On average the wives in our sample engage in 2.27 acts (s.d. = 1.62), and the husbands in 2.42 (s.d. = 1.76) on this scale (one-tailed $p < 0.05$).

For explanatory variables, we begin with a set of basic participatory factors—resources and engagement—and add to them measures of the various aspects of family equality just discussed. Measures of the participatory resources are education (coded from 1 to 6, ranging from 8th grade or less to graduate school) and family income in tens of thousands of dollars (0 to 25 that is, \$0 to \$250,000). To capture an individual's engagement with politics, we include as well a measure of political interest (coded from 1 to 4, ranging from not at all interested to very interested). These variables have been shown to be significant predictors of political

TABLE 6. Equality at Home: Overall Political Activity by Individual and Family Variables, OLS Regression Results

	Wives			Husbands		
	B	Standard Error	Beta	B	Standard Error	Beta
Participatory factors						
Education[a]	.29	(.09)**	.21	.28	(.08)**	.21
Family income[b]	.11	(.03)**	.24	.04	(.03)	.03
Political interest[a]	.36	(.12)**	.17	.54	(.14)**	.21
Domestic hierarchy: Relative family resources						
Percentage of family income brought in[a]	.005	(.004)	.08	.004	(.004)	.05
Advantage in job level over that of spouse[c]	−.02	(.05)	−.02	.07	(.06)	.08
Percentage of household work done[a]	.005	(.006)	.05	−.01	(.007)	−.10
Domestic hierarchy: Relative family power						
Discretion with respect to small amount of time[c]	−.15	(.15)	.05	.41	(.17)*	.14
Control over major financial decisions[c]	−.68	(.73)	.05	1.52	(.77)*	.11
Domestic hierarchy: Relative respect						
Gets more respect as advisor from spouse than gives in return[c]	.21	(.10)*	.11	−.02	(.11)	−.01
Beliefs in gender equality at home						
Wife's beliefs	.66	(.32)*	.13	.11	(.33)	.02
Husband's beliefs	−.08	(.50)	.00	−.04	(.54)	−.00
Constant	−2.97	(1.66)		−1.43	(1.74)	
N		271		270		
Adjusted R^2		.24		.20		

* $p < .05$. ** $p < .001$.

[a] Separate measure for each spouse.

[b] Same measure for each spouse.

[c] Same relative measure in each column; direction reversed to favor each spouse (e.g., wife's job level minus husband's for her; husband's minus wife's for him).

participation (Verba, Schlozman, and Brady 1995), and they should provide controls to ensure that any seeming effects of inequalities between wife and husband do not merely reflect characteristics of the individual or other social characteristics of the family.

To this base, we add five variables measuring family hierarchy, that is, the *relative position* of husband and wife with respect to class, status, and power *within the home*. (1) Money and time: the proportion of family income attributable to the wife (0 to 100) and the proportion contributed by the wife of the total hours devoted by the couple to housework (0 to 100). (2) Occupation status: the difference between the job levels of the wife and husband (−5 to 5). (3) Decisional control over money and time: relative control over major financial decisions (−1 to 1) and relative discretion with respect to use of small blocks of time (−1 to 1). (4) Respect: relative ranking of husband and wife on the list of trusted advisors (−2 to 2). (5) We also include a measure of beliefs about gender equality: both spouses' views about appropriate domestic roles for husband and wife (1 to 4, ranging from most traditional to least traditional).[18]

Thus, we seek to learn how hierarchy within the home with respect to such matters as resources brought in, control over resources available within the home, and relative respect as well as both spouses' beliefs about equality at home affect the participation of each member of the couple—over and above the effect of the basic participatory factors. Since we are concerned to understand how domestic hierarchy is related to political participation for each partner in the couple, we estimate separate models for wives and husbands. To help clarify what we are doing in these models, we code the relative variables in a direction favorable to the husband (e.g., the proportion of money he brings into the household) in the husbands' model and in a direction favorable to the wife in the wives' model. Table 6, which contains the results of these analyses, confirms that education and political interest are participatory factors for both husbands and wives, as is family income for wives. Our main concern, however, is with how various kinds of inequality between spouses, as indexed by our relative measures, build upon participatory predispositions rooted in an individual's education, income, and political interest.

Domestic arrangements do have implications for political activity. These implications, which are different for husbands and wives, do not conform to what we anticipated. Many of the factors that we expected to enhance wives' activity—for example, bringing in a high proportion of family income, dividing the housework relatively equally, or exercising authority over family time or money—seem not to affect their participation. Yet, wives do get an extra participatory boost from the relative respect they command within the household, that is, by being ranked higher on their husband's list of trusted advisors than they rank their husband. In addition, a wife who believes in equality at home is likely to be more active in politics, regardless of what her husband believes about domestic roles.

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[18] Our survey contains a number of different measures of these constructs. We used LISREL to determine the measure with the highest validity among those for a particular construct (Bollen 1989). We use that variable here to measure the construct. In three instances, we used regression measurement models to help reduce the amount of measurement error in the variables. In these three instances, no single measure was able to capture the construct sufficiently. These three variables—time given to work at home, control over money, and beliefs about gender equality—retain their original scales but contain less measurement error than their original versions. See Appendix C for elaboration.

The pattern is very different for husbands. Neither their own beliefs about appropriate domestic arrangements nor their wives' views have consequences for husbands' political activity. When it comes to the husband's participation, what seems to matter is whether he is boss at home. Husbands who exercise greater power over the family exchequer and who have relative autonomy in the use of small amounts of time are more likely to be active in politics.[19] In short, an egalitarian arrangement does not empower a husband for politics; exercising power at home does.

Since the results reported in Table 6 are central to our argument, we estimated the model that produced those results in various ways to test for its robustness, that is, to ensure that the signs for variables remain the same and the relationship of coefficients to their standard errors also remain similar though various specifications. We used a number of different indicators for the independent variables. For example, we estimated versions of the model in which we included measures of two additional participatory resources: job level and leisure time. As expected, neither variable has a significant effect on the activity of either husbands or wives, and the results presented in Table 6 were unchanged.[20] We also included a measure of the traditional division of labor, defined as a situation in which the wife does a disproportionate share of the shopping and cleaning. In none of the various specifications we tried did traditional division of labor have a significant effect either for women or men. Moreover, inclusion of this variable leaves other results unchanged.

Indeed, we could find no specification in which any aspect of relative availability of free time, division of labor with respect to housework, or the assumption of responsibility for any particular household task had a significant effect on wives' participation in politics.[21] These results, although surprising, parallel earlier findings to the effect that women are not, in the aggregate, deprived when it comes to the availability of free time and that, contrary to expectation, lack of time is not a substantial constraint on political action (Schlozman, Burns, and Verba 1994).[22] We also respecified the equation using alternative measures of the difference between husbands' and wives' estimates of family characteristics or using the two spouses' estimates separately. The basic results remain unchanged. In addition, when we introduced the individual-level variables along with the relative measures (that is, measures combining the responses of both marriage partners), the results did not change.[23]

We also replicated the analysis for different dependent variables. We distinguished, for instance, between two kinds of activity: that involving inputs of time and that involving inputs of money. Predicting activity that involves giving time yields a result very close to that presented in Table 6. Since the gender gap in financial contributions is especially pronounced, and since the single participatory factor that drives the size of political contributions is family income, it seemed important to probe this distinction further. (For discussion of political contributions and confirmation of the dominant role of family income, see Verba, Schlozman, and Brady 1995, chapter 12.) Interestingly, variables measuring inequality at home, including the proportion of overall family income attributable to the wife as well as a measure of control over family income, seem to have no effect on the size of political contributions.[24] For both husbands and wives, what matters for the size of political contributions is family income.

The data in Table 7 illustrate more concretely the significance of domestic arrangements for the amount of political participation by a wife or husband. The entries indicate the maximum possible effect of each variable shown to be statistically significant in Table 6.[25] These data confirm that, for both sexes, individual participatory factors—education, political interest, and, for women, income—are critical for political participation. Beyond this, it is clear that, when it comes to political activity, family hierarchy matters for both marriage partners, although different aspects of that hierarchy matter for wives and husbands. What is so striking about the findings in Table 7 is that the potential consequences of domestic hierarchy for husbands' participation are greater than the effects for wives.

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[19] In this context recall from Table 4 that, compared to wives, husbands are more likely to be dominant over family money than over their own time.

[20] Yet, the introduction of still more variables into an analysis that is based on a relatively small number of cases and that already contains many variables has the effect of reducing the *t*-statistic for the coefficient on the husbands' control over major financial decisions to 1.70 and the *t*-statistic for the coefficient on the relative respect enjoyed by wives to 1.88.

[21] In testing alternative specifications, we also considered whether small children, high-powered jobs, and day care change the effect of the division of labor and the control over time on women's participation.

[22] We should recall that, while in the aggregate men and women enjoy equal amounts of leisure, women with full-time jobs and children, especially preschool children, have less free time than do men in these circumstances. Our earlier work showed, however, that preschool children dampen women's and men's political involvement by an equal amount.

[23] It could be argued that the appropriate specification of the model is to use only individual-level measures rather than the relative measures. For the results of analyses based on individual-level measures, see Appendix D.

[24] One reason may be that financial contributions tend to be made jointly by both family members.

[25] The maximum effect was calculated by multiplying the regression coefficient from Table 6 by the difference between the highest and lowest values the associated variable takes in our data.

TABLE 7. The Maximum Possible Effect on Overall Political Participation of Participatory Factors

Effect of Various Factors on	Maximum Effect
Wives' political participation	Acts
Participatory factors	
Education	+1.45
Family income	+2.75
Political interest	+1.08
Total effect of participatory factors	+5.28
Domestic hierarchy	
Relative respect as an advisor	+0.84
Beliefs	
Her belief in equality at home	+0.96
Husbands' political participation	
Participatory factors	
Education	+1.40
Political interest	+1.62
Total effect of participatory factors	+3.02
Domestic hierarchy	
Relative control over small amounts of time	+0.82
Relative control over financial decisions	+0.59
Total effect of family hierarchy	+1.41

Note: Overall Participation Scale range: 0 to 8.

The data in Table 7 document the maximum possible effect on the political activity of an *individual* wife or husband of changes in the various factors that influence participation. We are also interested in how these factors affect the aggregate political activity of husbands and wives across all the couples. From the latter perspective, Table 7 must be read in the context of how these factors are actually distributed between wives and husbands. Consider the measure of the relative respect that the wife receives at home. As we saw in Table 6, if she receives more respect (relative to the amount she accords her husband), her activity level is boosted; if she receives less respect, her activity level is correspondingly depressed. As noted earlier, 40% of couples demonstrate asymmetry of respect: In 22% of the cases, he gets more respect than he gives; in 18% she gets more than she gives. Thus, there is a fairly even balance between the spouses when it comes to enjoying relative respect; indeed, the balance tips ever so slightly in favor of the husbands. What this means is that, although enjoying relative respect raises the participation of those wives who receive it, when aggregated across all couples the effect is almost negligible. In contrast, the wife's belief in gender equality—an individual measure that does not depend on her beliefs relative to her husband's—does have the effect of increasing her political activity.

The factors that influence a husband's participation show different patterns. His political activity is enhanced to the extent that he controls both spouses' time or the family finances. In the aggregate, however, husbands are disadvantaged with respect to the former and—to an even greater degree—advantaged with respect to the latter (Table 4). Hence, in the aggregate, the constraints that husbands express regarding their ability to use time diminish their aggregate participation, while their command of the family finances enhances that aggregate activity.[26] The countervailing forces are not equal, however. Across all couples, husbands gain about twice as much in aggregate activity from their greater control over family finances than they lose by their somewhat lesser control over their free time.[27]

What does this mean for the balance of participation as between husbands and wives across all couples? If we take into account the aggregate effect of family hierarchy on the participation of wives (which is negligible) and the aggregate effect of family hierarchy on husbands (which is positive because the boost from their greater control over family finances more than compensates for the loss from their lesser control over time), then we see that domestic inequality works to advantage the participation of husbands.

In sum, we have shown the following: Relative position in various family hierarchies can affect the participation of an individual husband or wife; husbands and wives differ with respect to the aspects of domestic hierarchy that have significant consequences for participation; and the average effect of the various aspects of domestic inequality on the activity of husbands and wives depends also on the extent to which, across couples, one partner or the other enjoys advantage. In addition, these data demonstrate unambiguously the proposition that the effects on political participation of inequality at home cannot be fully understood without considering both partners.

CONCLUSION

This analysis has subjected to rare empirical test the widely held view that what happens in the private domain of the home has consequences for participation in politics. We consider it a major step forward to have used data drawn from a random sample to probe a series of critical issues often subject to speculation without benefit of systematic empirical evidence. Our case base is smaller than we would like, and because respondents who can be found for the third wave of a study may not be typical of those who cannot be located a third time, we present our findings cautiously. Nevertheless, we believe the data confirm the fact that private life in the family has consequences for the public involvement of family members. We hope this inquiry will trigger on ongoing conversation, grounded in systematic data, about the relationship between inequality at home and citizen politics.

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[26] The means of the relative measures are more descriptive in regard to relative respect and relative control over time. They are less useful—the distributions are quite flat—in regard to control over money.

[27] The aggregate average effect of control over money is .23 (1.52 [the coefficient] × .15 [the average value]); the aggregate average effect of control over time is −.10 (.41 [the coefficient] × −.24 [the average value]).

This analysis places a new spin on the usual interpretation of how family life operates to place women at a disadvantage in politics. Most important, domestic arrangements affect the political activity of both marriage partners. Construing domestic inequalities very broadly to encompass relative economic wherewithal, decision-making power, and social status, our data indicate that domestic hierarchies have greater implications for the political activity of husbands than of wives. The particular aspects of domestic inequality that matter for the political activity of husbands as individuals are his control over major financial decisions and his relative autonomy in using small amounts of time; the former increase, as well, the aggregate of husbandly participation. These factors are often cited as placing married women at a disadvantage, but they do not, in fact, have the expected depressing effect on wives' political activity. Rather, these factors boost the activity of their husbands. Thus, insofar as family inequality has an effect on the gender gap in political activity, it does so, as predicted by feminist theorists, through male dominance at home. Yet, the domestic power of dominant husbands does not depress the participation of wives; rather, it boosts the activity of husbands beyond what would be expected on the basis of their other characteristics.

The findings for women contradict many of the expectations generated by the literature. Try as we might, we could find no evidence that absence of free time handicaps women as citizens. Previous research had demonstrated both that, unlike other tangible resources such as money or politically relevant skills, time does not play a strong role in facilitating political participation and that women are not, in the aggregate, disadvantaged when it comes to free time. Building upon these results, we investigated time as a resource within the household. Our analysis could not confirm that the relative amounts of leisure enjoyed by husband and wife have any effect on the political activity of either one. In addition, we were surprised to find that, in the aggregate, wives feel somewhat more autonomous than husbands in using small amounts of time. Yet, that autonomy does not seem to translate into political activity—in contrast to the situation for husbands, who increase political activity when they feel free to use their spare time as they wish. Moreover, we could find no evidence to support the corollary hypothesis that wives are deterred from political participation by the undeniable fact that they assume a disproportionate share of the responsibility for running the household. Neither the absolute amount of time each partner gives to housework, nor the relative contribution of each, nor the assumption of responsibility for particular household tasks affects the political activity of either husband or wife.

When it comes to the factors that affect the activity of women, our results are striking and somewhat unexpected. Research on citizen participation has demonstrated over and over that resources such as money, education, and civic skills enhance activity. Yet, once we pass over the threshold into the privacy of the home, what is politically enabling for wives is not the household equivalent of such tangible resources. Instead, the less tangible aspects of a couple's domestic life seem to affect wives' activity. Having or controlling the stockpile of family time or money is not what counts. Rather, the wife who enjoys respect from her husband and who believes in equality of the sexes at home is, all else equal, likely to be somewhat more active in politics. Thus, control over resources matter for men; beliefs about equality matter for women.

APPENDIX A: THE SAMPLE

The data for the first two waves of the sample come from a two-stage survey of the voluntary activity of the American public. The first stage consisted of 15,053 telephone interviews of adult (18 years old or older) Americans conducted by the Public Opinion Laboratory of Northern Illinois University and the National Opinion Research Center (NORC) during the last six months of 1989. These interviews were between 15 and 20 minutes in length, and they provided a profile of political and nonpolitical activity as well as basic demographic information. Respondents were selected randomly from telephone exchanges matched to the primary sampling units of the NORC national, in-person sampling frame. This clustered sample was designed to be representative of the American population. Within each household, adults were chosen at random using a Kish table.

The sample for the second wave was designed to incorporate distinctive features that meet the particular requirements of studying participation and representation. To assess how well the activists reflect the citizenry, a representative sample of the public is essential. Yet, ordinary representative samples contain very few of the most interesting activists—those who engage in relatively rare but important activities, such as giving large donations, serving on local governing boards, or taking part in protests. Similarly, they contain few cases of activists drawn from politically relevant racial and ethnic minorities—African Americans and Latinos—groups of particular interest in an investigation of participatory representation. To select respondents for the second stage of in-person interviews, the sample of 15,053 was first reweighted to adjust for the fact that the screener had yielded a slightly disproportionate share of women. The sample was then stratified by race and ethnicity (African American, Latino, and "all other") and by level and type of political participation. African Americans, Latinos, and political activists were oversampled, with weights ranging from 1 (inactive Anglo-whites) to 16 (highly active Latinos). In spring 1990, NORC conducted in-person interviews averaging almost two hours each with 2,517 of the original 15,000 respondents. With appropriate sampling weights, these data can be treated as a national random sample.

The data from the first two waves are deposited at the Inter-university Consortium for Political and Social Research at the University of Michigan (Verba et al. 1990). A codebook is available for each survey, along with extensive information on sampling and other technical matters.

For the third wave, we interviews all traceable respondents from the second wave. When they happened to be married, we interviewed their spouse as well. We were able to reinterview 609 of the second-wave respondents, 380 of whom were married at the time of the third-wave interview. For married respondents, we conducted separate full interviews with the spouse. With appropriate sampling weights, the data from the third wave can be treated as an ordinary random sample. As for the previous two waves, this is a national sample.

APPENDIX B: QUESTION WORDING AND SCALES

Time Spent on Household Chores

"About how many hours per day do you spend on necessary work for your home and family, including cooking, cleaning, taking care of children or other relatives, shopping, house and yard chores, and so forth? About how many hours in total do you spend in an average day on such necessary activities for home and family?" We asked a parallel question about the respondents' estimates of their spouse's time spent on household chores.

For the wife's report of the percentage of the household time she contributed, we used the number of hours she ascribed to her own work expressed as a percentage of the total number of hours she estimated that she and her husband spent, individually, on household work; for example, a wife who reported that she spent four hours and her husband spent one hour was coded 100 × (4/(4 + 1)), or 80.

For the husband's report of the percentage of the household time his wife contributed, we used the number of hours he ascribed to her work expressed as a percentage of the total number of hours he estimated that he and his wife spent, individually, on household work; for example, a husband who reported that he spent four hours and his wife spent one hour was coded 100 × (1/(4 + 1)), or 20.

For the combined measure that we used in the regressions, we used the number of hours that she ascribed to her work expressed as a percentage of the total number of hours she estimated that she spent and he estimated that he spent, individually, on household work; for example, a wife who reported that she spent four hours and a husband who reported that he spent four hours was coded 100 × (4/(4 + 4)), or 50.

Income

"Now, look at response card H. (The income categories from response card H are as follows: A. Under $5,000; B. $5,000–9,999; C. $10,000–14,999; D. $15,000–19,999; E. $20,000–24,999; F. $25,000–29,999; G. $30,000–34,999; H. $35,000–39,999; I. $40,000–49,999; J. $50,000–59,999; K. $60,000–74,999; L. $75,000–99,999; M. $100,000–124,999; N. $125,000–149,999; O. $150,000–199,999; P. $200,000 and over.) Which of the income groups listed on this card includes the *total income* before taxes of all members of your family living in your home? Please include salaries, wages, pensions, dividends, interest, and all other income. Please tell me the letter for the correct category.

"Look, again, at card H. In which of these groups did *your own* total earnings from your occupation fall last year [1992] before taxes. Just tell me the letter.

"Besides income from wages, did you *yourself* bring other income into the household this past year, for example, from child support, alimony, governmental benefits, interest earnings on your own accounts, rent on your own real estate, any inheritances, or the like?

"[If other income] Look at card H one more time. Which letter best represents the amount of money you *yourself* brought into the household, from these sources *other than your wages* in 1992?"

We recoded these variables to indicate tens of thousands of dollars.

For the descriptive statistics, we used information from each spouse to calculate each spouse's estimate of the wife's contribution to family money.

For the wife's estimate of her contribution to the family income, we used the income that she ascribed to herself from her job and from other sources as a percentage of her estimate of the total family income, for example, a wife who reported bringing in $10,000 from her job and $10,000 from other sources and who reported a family income of $40,000 was coded 100 × ((1 + 1)/4), or 50.

For the husband's estimate of his wife's contribution to the family income, we used the income that he ascribed to himself from his job and from other sources as a percentage of his estimate of the family income; to calculate his estimate of her contribution, we subtracted his estimate of his contribution from 100. So a husband who reported bringing in $10,000 from his job and $10,000 from other sources and who reported a family income of $40,000 was coded 100 − (100 × ((1 + 1)/4) = 100 − 50, or 50.

For the combined measure of the wife's contribution to the family income, we used the income that she ascribed to herself from her job and from other sources as a percentage of that total income plus the income that he ascribed to himself from his job and from other sources; for example, a wife who reported bringing in $10,000 from her job and $10,000 from other sources and a husband who reported brining in $20,000 from his job and $20,000 from other sources would be coded 100 × ((1 + 1)/(1 + 1 + 2 + 2)), or 33.

Division of Household Chores

"Now I'd like to ask you some questions about activities around the house. I am going to read you a list of some chores that usually need to be done in a household. After I read each one, please refer to response card E and reply with the *letter* that best represents your answer. If nobody does it, or if you pay someone to do it, just tell me.

"What about grocery shopping? How much responsibility do you take for this? All, most, some, a little, or none? Just read me the letter.

"What about paying family bills, that is, actually writing the checks? How much responsibility do you take for this? Just read me the letter.

"How about repairing the car or taking the car in for repair? How much responsibility do you take for this? Please read the letter from card E that describes your choice?

"What about house-cleaning chores such as vacuuming, dusting, and cleaning the bathroom? How much responsibility do you take for this? Please read a letter from card E that is next to your choice.

"[If there are children at home] What about taking care of children, that is, all child-related activities, including taking care of them, driving them places, and helping them with homework, for example? How much responsibility do you take for this? Please read me the letter from card E next to your choice."

Control over Time and Money

"Think about bigger financial decisions—things like how much your family can afford to spend on housing or how much, and what kind, of insurance to buy. How much responsibility do you take for this? Please read me the letter from card E next to your choice."

For the regressions, we combined the two spouses' responses such that 1 indicated that he had more control (and the two spouses agreed about that), 0 indicated that they were equally in control (or that there was credit claiming), and −1 indicated that she had more control (and the two spouses agreed about that). We cleaned this measure as we describe in Appendix C.

"Now I'd like to ask you a few questions about things families usually have to make decisions about. For each question use response card G to tell me whether this is a decision you would make on your own or whether your husband (wife) would have some say in the decision. What about whether to give a few hours (commit time on a regular basis) to a charity drive (something you enjoy that doesn't involve your husband [wife]/a political campaign or cause)?"

For the regressions, we combined the two spouses' responses such that 1 indicated that he had more autonomy than she did, 0 indicated equality, and −1 indicated that she had more autonomy than he did.

Respect in the Family

"From time to time, most people seek out other people whose judgment they trust to discuss important matters. Think about *three* people whose judgment you really trust, and with whom you might be likely to discuss important matters. Please use the categories on response card C.

"Think of the *first* (second/third) person you'd be likely to talk with. Which category on card C describes that person? Please read the letter next to your choice.

A. An advisor—for example, a lawyer, a doctor, or a priest, minister, or rabbi
B. A friend or neighbor
C. A co-worker
D. Your wife (husband)
E. Another close relative besides your wife (husband) (e.g., mother, father, or one of your siblings or children)
F. Someone else"

We first took each respondent's combined responses to the three questions to create a variable that we coded 1 if the respondent would turn first to the spouse, 2 if the respondent would turn second or third to the spouse, and 3 if the respondent would not turn to the spouse first, second, or third. Then we combined the two spouses' scores such that 2 indicated that he would turn to her before she would turn to him, 0 indicated equality, and −2 indicated that she would turn to him before he would turn to her.

Beliefs about Gender Equality

"Now, on a different topic, I am going to read you a series of statements and for each one, please use card F to tell me whether you agree strongly, agree, disagree, disagree strongly, or have no opinion.

A. A wife should give up her job whenever it interferes with her roles as a wife and mother.
B. Husbands and wives should share household chores equally.
C. It's best if just the husband is in charge of making family decisions."

For the regressions, we recoded the three variables such that higher numbers indicated a stronger belief in gender equality. We calculated an average score (on the four-point scales) for each respondent. We cleaned the variables as we described in Appendix C.

Job Level Scale

The job level variable is based on the amount of education and on-the-job training the respondent believes is needed for the respondent's job. It has five levels. The lowest level jobs require no more than a high school diploma and no more than one month of on-the-job training for mastery. At the other end of the scale (level 5) are jobs requiring either a college degree and at least two years of training on the job or a graduate degree.

The questions asked were: "(In general) How much formal education does somebody need to do a job like (yours/the one you had)—no special formal education, a high school diploma, technical school, a college degree, a graduate degree, or what?" and "How long does a person have to spend in training on the job to be able to handle a job like yours?"

Although the job level classifications are based on what respondents told us rather than upon objective assessments, examination of actual cases suggests that respondents make judgments fairly accurately. Examples of occupations at each level include:

1. Dishwasher, janitor, cashier
2. Bank teller, mail carrier, machine operator
3. Electrician, machinist, construction inspector
4. Insurance agent, engineer, elementary school teacher
5. Physician, architect, attorney

To calculate job level difference, we subtracted her job level from his.

Education

The education variable has six categories:

1. 8th grade or lower
2. 9–12
3. graduated high school or GED
4. some college
5. college graduate
6. graduate school

The following questions were used to create the categorization.

"What is the highest grade of regular school that you have completed and gotten credit for? (IF NECESSARY, SAY: By regular school we mean a school which can be counted toward an elementary or high school diploma or a college or university degree.)

"Did you get a high school diploma or pass a high school equivalency test?

"Do you have any college degrees, that is, not including degrees from a business college, technical college, or vocational school?

"What is the highest degree that you have ever received?"

Political Interest

The political interest scale is based on the following question: "How interested are you in politics and public affairs? Are you very interested, somewhat interested, only slightly interested, or not at all interested in politics and public affairs?" The variable is coded 1 to 4, with 4 indicating very interested.

Political Activity Scale

Eight political acts were added together to form our overall scale.

Voting. A voter is defined as someone who voted in the 1992 presidential election. The following questions were asked.

"Are you currently registered to vote?

"In talking to people about elections, we find that they are sometimes not able to vote because they're not registered, they don't have time, or they have difficulty getting to the polls. Think about the presidential elections since you were old enough to vote. Have you voted in all of them, in most of them, in some of them, rarely voted in them, or have you never voted in a presidential election?

"Thinking back to the national election in November 1992, when the presidential candidates were Bill Clinton, the Democrat, and George Bush, the Republican, did you happen to vote in that election?"

Campaign Work. "Since January 1992, the start of the last national election year, have you worked as a volunteer, that is, for no pay at all or for only a token amount, for a candidate running for national, state, or local office?"

Campaign Contributions. "(We have been talking about campaign activity.) Now we would like to talk about contributions to campaigns. Since January 1992 did you contribute money to an individual candidate, a party group, a political action committee, or any other organization that supported candidates?"

Community Activity. "Now some questions about your role in your community. In the past *two years*, since [current month] have you served in a voluntary capacity, that is, for no pay at all or for only a token amount, on any official local governmental board or council that deals with community problems and issues, such as a town council, a school board, a zoning board, a planning board, or the like?

"Aside from membership on a board or council or attendance at meetings, I'd like to ask also about informal activity in your community or neighborhood. In the past twelve months, have you gotten together *informally* with or worked with others in your community or neighborhood to try to deal with some community issue or problem?

Contacting. "In the past five years, since [interview date 1992], have you initiated any contacts either in person, by phone, or by letter with a government official on the national, state, or local level about a problem or an issue of concern to you? Was this [most recent] contact in the past twelve months?

Protesting. "In the past two years, since [current month], have you taken part in a protest, march, or demonstration on some national or local issue (other than a strike against your employer)?"

Political Organization. "There are many kinds of organizations that people join, for example, unions or professional associations, fraternal groups, recreational organizations, political issue organizations, community or school groups, and so on. *Not including* membership in a local church or synagogue, are you a member of any organizations? Do any of these organizations ever take stands on any public issues, either local or national?"

We asked a parallel question of those who were members of a single organization. We coded the respondent as belonging to a political organization if s/he answered yes to the question about whether any of the organizations take stands in politics.

The Overall Political Activity Index

One point was given for each act. The additive scale runs from 0 to 8.

The various items of the index are all positively related to one another, as the following correlation matrix shows:

Items in Overall Political Activity Index[a]

	Correlation Coefficients						
	Campaign Work	Campaign Contribution	Contact	Protest	Community Board	Informal Community	Political Org.
Vote	.15	.28	.22	.04	.10	.13	.26
Campaign Work		.27	.21	.13	.16	.13	.18
Campaign Contribution			.26	.15	.13	.15	.37
Contact				.15	.16	.23	.31
Protest					.08	.10	.16
Community Board						.15	.12
Informal Community							.19

Note: All correlations are significant at <.05.
[a] Cronbach's $\alpha = .6192$

APPENDIX C

Our survey contained a number of different measures of the equality dimensions discussed in this article. We used LISREL to determine the measure of each of these constructs with the highest validity (in other words, the measure with the largest coefficient on the underlying construct) (Bollen 1989). In four cases, no single measure was able to capture the construct sufficiently: percentage of housework hours put in by the wife, control over major financial decisions, his belief in gender equality, and her belief in gender equality. As a consequence, we developed measurement models for these four variables. To construct these cleaned measures, we first used LISREL to select the measure with the highest validity. We then regressed that variable on the other measures of the construct; the predicted values from those equations are the variables we used here. Our goal was to take advantage of the common variance across the measures for each construct. We chose this path instead of the creation of a scale in order to keep the variables in their original scales. The consequent measures do retain their original scales, but they contain less measurement error than their original versions.

For the measure of the percentage of housework hours put in the wife (described in Appendix B), we regressed her percentage of the hours using information from both the husband and the wife on the difference between the wife's hours and the husband's hours using combined estimates, the wife's percentage using only information from the wife, the wife's percentage using only information from the husband, a combined report of who does the housecleaning, and a combined report of who does the grocery shopping. The resulting estimation had a corrected R^2 of .81. The coefficients and standard errors for the equation were:

Difference in hours	−2.85 (.16)
Wife's estimate	0.13 (.03)
Husband's estimate	0.25 (.03)
Housecleaning	0.004 (.86)
Grocery shopping	−1.09 (.75)
Constant	30.42 (.03)

For the measure of control over family finances, we regressed a combined measure of that control on a combined measure of that control on a combined measure of who pays the bills and relative autonomy with respect to spending small amounts of money. The corrected R^2 was .07. The coefficients and standard errors for the equation were:

Paying bills	0.13 (.03)
Relative autonomy with respect to spending small amounts of money	0.06 (0.035)
Constant	0.19 (0.02)

For the two measures of his belief in gender equality and the measure of her belief in gender equality, we used an average of each spouse's responses on three questions as the dependent variable in the measurement equation: whether a wife should give up her job whenever it interferes with her roles as wife and mother, whether husbands and wives should share chores equally, and whether it is best if just the husband is in charge of making family decisions. For each spouse, we regressed this averaged variable on whether a woman's place is in the house, whether the government should help women, whether men have more of the top jobs because they are born with more drive and ambition, whether an employed mother can establish just as warm and secure a relationship with her children as a mother who is at home full time, whether a mother of young children should work if the family does not need the money, whether the husband should have major responsibility for making financial plans, and whether it is anyone else's business if a husband hits his wife from time to time. The adjusted R^2 for this equation was .38 for women and .17 for men. The coefficients and standard errors are given below.

	Her Belief	His Belief
Should have equal role	.08 (.02)	.04 (.02)
Women should help themselves	−.03 (.01)	.00 (.01)
Men have top jobs because they are more ambitious	−.07 (.02)	−.05 (.02)
Employed mother cannot have warm relationship with children	.07 (.03)	.04 (.03)
It is others' business if husband hits wife	.15 (.05)	.16 (.05)
A mother of young children can work if she wants	.12 (.02)	.10 (.03)
Man should not have the major responsibility for the couple's financial plans	.05 (.02)	.07 (.03)
Constant	1.63 (.22)	1.69 (.24)

APPENDIX D

Since it could be argued that the correct measures would be individual-level measures rather than the relative measures that we used in the article, we report an analysis using individual measures here. For all but one of the relative measures, we also have individual-level measures. Our goal in these equations was to choose the set of measures that demonstrated the highest level of stability across different specifications. The contextual measures were significantly more stable than were the individual-level measures. Thus, they won out in our specification efforts. Nevertheless, we wish to provide a sense of the extent to which the individual-level measures drive the results we report here. In this appendix, we report the individual-level coefficients and their standard errors. We use the model reported in Table 6 for these estimations. We substitute the particular individual-level variables for the contextual variable in the equation. We do this one variable at a time.

For income, we added her income from all sources to the model; the variable for the family income is already in the model. For her equation, the coefficient on this variable is 0.15, with a standard error of .07. For his equation, the coefficient on this variable is −.20, with a standard error of .07. These significant coefficients are not highly stable (see Burns 1995).

For job level, we included each spouse's job level. In her equation, the estimate for his job level is .01 (.07) and for her job level is .05 (.08). In his equation, the estimate is .08 (.09) for his job level and −.06 (.07) for hers. These are not systematic.

For time spent in housework, we included the number of hours each spouse said s/he contributed each day. In her equation, the estimate for his contribution is −.07 (.05) and for her contribution is −.004 (.03). In his equation, the estimate for his contribution is .002 (.05), while the estimate for her contribution is .05 (.03). These are not systematic.

For autonomy with respect to time, we used each spouse's report of her or his own autonomy. In her equation, the estimate for her lack of autonomy is −.09 (.11) and for his is −.10 (.08). In his equation, the estimate for her lack of autonomy is −.06 (.12) and for his is −.23 (.08). While the combined report is more stable, this result suggests that the effect of the autonomy measure for men is driven by their own lack of autonomy and not by the comparison between the two spouses' autonomy.

For consulting with the spouse, we used each spouse's report of her or his consulting behavior (0–2, with 2 indicating that the spouse is the first person turned to). In her equation, the estimate for his behavior is .002 (.14) and for hers is −.42 (.14). In his equation, the estimate for his behavior is .008 (.15) and for hers is −.03 (.15). Again, while the combined report generates the most stable coefficient, this result suggests that the effect of the confiding measure for women may be driven by women's own behavior. The relationship is complex. Women who confide in their spouse tend to believe in and practice equality at home. In general, then, because women who confide in their spouse tend to believe in equality, the effect is a wash (the combination of this coefficient and the coefficient on belief in equality). Yet, when women who do not believe in equality confide heavily in their husband, they participate less. The first group of women mentioned practices a companionate marriage. The second group confides because these women believe in inequality. The consequences are quite different for the two groups of women.

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The Quixotic Search for Consensus on the U.S. Supreme Court: A Cross-Judicial Empirical Analysis of the Rehnquist Court Justices

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In this first systematic and extensive application of cross-judicial methodology, we examine the members of the Rehnquist Court (1986–94 terms) with prior appellate court experience to discern any correlation with their Supreme Court behavior in terms of nonconsensual opinion writing and voting. We find that they become less consensual as justices than they were as judges in the lower court. Importantly, this finding holds after controlling for such institutional differences between the two court levels as size, ideology, case types, stare decisis, and norms. Consistent with the neoinstitutional perspective, we surmise that this behavior change is due to the modern Supreme Court being unique, a court on which the members feel it is desirable, necessary, and possible to express policy disagreements with the majority via separate opinions and votes.

When John Marshall was appointed chief justice of the U.S. Supreme Court in 1801, he put an end to the practice of each justice issuing his own opinion, or "seriatim" opinion writing. Marshall did this because he believed that the Supreme Court's "power and prestige" would be enhanced if it spoke with a "single voice" (Abraham 1993, 199). For the past fifty years or so, however, nonconsensual opinion writing and voting behavior by the justices has been the rule rather than the exception (e.g., Walker, Epstein, and Dixon 1988). Importantly, and as John Marshall surely would have understood, the increased display of conflict by *the justices* has had profound implications for the U.S. political system because it has made *the Court's* authority more easily subject to challenge (see, e.g., Johnson and Canon 1984, O'Brien 1996).

Political scientists long have been interested in the sources of conflict in Supreme Court decision making. Walker, Epstein, and Dixon (1988) assessed much of the previous literature and, using both historical and quantitative methods, evaluated the most frequently suggested reasons for the demise of consensus among the justices: the Court's discretionary jurisdiction, changing caseload, associate justice characteristics, and judicial leadership. They concluded that "in combination with other factors the leadership style introduced by Harlan Fiske Stone in 1941 had a dramatic effect on the consensus norms of the Court" (p. 361).

To date, the scholarship on the sources of conflict in the decision-making behavior of Supreme Court justices has focused exclusively on the Supreme Court. We take a broader perspective: a "cross-judicial" perspective.[1] In the mid-1980s, several prominent public law specialists began to apprise colleagues of the potential importance of cross-judicial empirical comparisons between the Supreme Court and the lower appellate courts (e.g., Gibson 1983, Goldman and Lamb 1986, Tate 1983; see also Baum 1994). Not until 1992 was the first such comparison conducted. In an article assessing Clarence Thomas's first year on the Supreme Court, Gerber performed a t-test comparing the number of opinions Thomas authored that year with the number he authored during his tenure on the U.S. Court of Appeals. The comparison was conducted to objectify the "initial bewilderment" component of the freshman effect hypothesis (Gerber 1992, 121–2).[2] Similarly, Hagle (1993, 1152–4) offered a cross-judicial voting behavior comparison in his more general article on the freshman (or acclimation) effect. He endeavored to ascertain whether a correlation exists between a justice experiencing acclimation effects on a lower appellate court and her or his experiencing acclimation effects on the Supreme Court. (He concluded that there was not a significant correlation.)

This paper expands on these efforts by offering the first systematic and extensive comparison between the behavior of U.S. Supreme Court justices and the behavior they exhibited while serving as lower appellate court judges. Our vehicle for doing this is the Rehnquist Court justices. Specifically, we examine the members of the Rehnquist Court (1986–94 terms) with prior appellate court experience to discern whether a Supreme Court justice's lower appellate court behavior correlates with her or his Supreme Court behavior, in terms of nonconsensual opinion writing and voting. Given that it is increasingly common for appointees to the Supreme Court to have had prior appellate court experience—of the fourteen individuals who served on the Rehnquist Court through the 1994 term, only three

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[1] "Cross-judicial" is used to avoid confusing comparisons *within* the U.S. judicial system with those made *between* the judicial systems of two or more nations. The latter type, logically termed "comparative-institutional" or "comparative-judicial," has a long and rich tradition in U.S. political science. (See, e.g., Abraham 1993.) The former type, as we note in the text, does not.

[2] The "initial bewilderment" component of the freshman effect hypothesis postulates that if a new justice is disoriented on the Supreme Court, s/he will be more reluctant to author opinions there than when s/he was on a lower appellate court. (See, e.g., Snyder 1958.) Gerber's 1992 cross-judicial comparison marked the first evaluation of the initial bewilderment component using other than anecdotal evidence (e.g., comments from a new justice such as Thomas about feeling "a little bit anxious or apprehensive" during the freshman term).

did not have such experience—we believe the time is right for public law scholars to engage in more cross-judicial empirical analysis.[3]

Although cross-judicial methodology can be productively applied to a host of substantive issues—witness, for example, Gerber's and Hagle's use of the methodology, limited though it was, on freshman effect issues—we focus on nonconsensus because in recent years considerable concern has been expressed about the implementation and compliance problems that result from noninstitutional, that is, nonunanimous, Supreme Court decisions. Who could not be concerned about decisions such as *Church of the Lukumi Babalu Aye, Inc.* v. *City of Hialeah* (1993)?

> Kennedy, J., delivered the opinion of the Court with respect to Parts I, III, and IV, in which Rehnquist, C.J., and White, Stevens, Scalia, Souter, and Thomas, JJ., joined, the opinion of the Court with respect to Part II-B, in which Rehnquist, C.J., and White, Stevens, Scalia, and Thomas, JJ., joined, the opinion of the Court with respect to Parts II-A-1 and II-A-3, in which Rehnquist, C.J., and Stevens, Scalia, and Thomas, JJ., joined, and an opinion with respect to Part II-A-2, in which Stevens, J., joined, Scalia, J., filed an opinion concurring in part and concurring in the judgment, in which Rehnquist, C.J., joined. Souter, J., filed an opinion concurring in the judgment. Blackmun, J., filed an opinion concurring in the judgment, in which O'Connor, J., joined.

After devoting extensive time to reading the separate opinions in decisions such as that (and that decision did not have a dissent!), one is then faced with the Herculean task of trying to determine whether an actual majority exists behind any proposition (for example, Justice Souter agreed with all but Part III-B) and, if so, what that proposition is (Horwitz 1993, 99). Justice Byron White (1993, vii) certainly was concerned: He ended his thirty-one year Supreme Court career by expressing the hope that future opinions would be "clear, crisp," and easy for lower court judges to understand and apply.

Using cross-judicial methodology to examine consensus/conflict is, therefore, of considerable practical consequence. Most notably, if our findings show a significant correlation exists between a justice's level of nonconsensual behavior on the Supreme Court and her or his level of nonconsensual behavior on a lower appellate court, scholars could conduct an analysis of a Supreme Court *nominee's* (or potential nominee's) lower appellate court behavior and make those findings available during the appointment process as a predictor of whether a particular individual is willing to work for consensus. If, however, no significant correlation exists between a justice's amount of nonconsensual behavior on the Supreme Court and her or his amount of nonconsensual behavior on a lower appellate court, we will have provided additional evidence of the importance of institutional constraints on judicial decision-making (see, e.g., Brace and Hall 1993, 1995; Gibson 1978, 1983, 1991; Hall and Brace 1989, 1996; see generally Smith 1988). Indeed, it is difficult to imagine a better public law test of the neoinstitutional perspective than a "natural experiment" that compares the behavior of the *same* individuals at *different* court levels.[4] President Clinton recently made a similar, albeit less scientific, comparison when he nominated Stephen Breyer to the Supreme Court in large part because of the nonpartisan "consensus-building" skills that Breyer allegedly exhibited on the U.S. Court of Appeals (Masci 1994).[5] Thus, using the Rehnquist Court justices as the subjects of our analysis should interest scholars and practitioners alike.

THE NONCONSENSUAL BEHAVIOR HYPOTHESIS

We test what we call the "nonconsensual behavior" hypothesis. This hypothesis, in question form, is: Can we predict, by examining a potential Supreme Court justice's lower appellate court opinion writing and voting behavior, whether s/he will be as consensus prone on the Supreme Court as s/he was on a lower appellate court?[6]

Public law scholars suspect that individuals become less consensus prone in the Marble Temple because the modern Supreme Court is a unique court, a court whose institutional environment leads to the overt expression of the justices' personal policy goals in their opinion writing and voting behavior (e.g., Goldman and Lamb 1986, O'Brien 1996).[7] That is to say, the Supreme Court is distinctive among collegial courts in the United States in the type and controversial nature of the cases it decides, in the greater freedom to depart from precedent it enjoys, in the number of judges involved in its decision-making process, and in the norm of individualism it invites.[8]

As President Clinton's recent observation about Breyer reveals, however, practitioners involved in the appointment process often make cross-judicial comparisons, albeit on a nonscientific basis. Many practitioners assume that a potential nominee to the Supreme Court will exhibit behavior patterns on the Supreme Court similar to those s/he exhibited on a lower court. In fact, some presidents have *insisted* on prior judicial experience as a prerequisite for considering someone for a Supreme Court appointment (e.g., Abraham 1993, 57–8). For instance, all four of the justices Dwight Eisenhower appointed after Earl Warren had such experience. The backgrounds of those who have served on the Rehnquist Court suggest that recent presidents have returned to the Eisenhower model: *Every* justice appointed to the Court after William Rehnquist's promotion to chief justice has had prior judicial experience, and the only two who served on the Rehnquist Court (other than Rehnquist himself) who did not have such experience were already on the Court before Rehnquist became chief justice. Of course, some presidents have been disappointed by the failure of their appointees to perform as expected, but this does not change the fact that prior judicial experience is often an important consideration in the appointment process—and an increasingly important consideration at that.

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[3] In his 1994 American Political Science Association presidential address, Charles Jones (1995) called for more cross-institutional and comparative-institutional analyses.

[4] For an interesting "natural experiment" involving House members who later became senators, see Grofman, Griffin, and Berry (1995).

[5] While filling in for President Clinton at Justice Breyer's swearing-in ceremony, Vice President Gore (1994) referred on more than one occasion to then Judge Breyer's "genius for building consensus."

[6] We adopt the convention of testing whether an individual is "more" or "less" consensus prone because of the subjectivity inherent in establishing an external standard for what constitutes "consensus" or "nonconsensus" (see, e.g., Howard 1981, 193; see generally Walker, Epstein, and Dixon 1988).

[7] Students of the U.S. Court of Appeals (e.g., Songer 1991), state supreme courts (e.g., Glick 1991), and intermediate state courts of appeals (e.g., Dubois 1988) have shown that judges on those courts have more discretion to impose their personal policy preferences on the cases that come before them than is suggested by low rates of dissent. Our research focuses on the *overt* manifestations of policy conflict—that is, concurring opinions and dissenting opinions and votes—that are of concern, for implementation and compliance reasons, to many justices (e.g., Byron White), practitioners (e.g., Bill Clinton), and public law scholars (e.g., David O'Brien). We discuss these implementation and compliance concerns in the conclusion.

[8] The U.S. Supreme Court and more than half the state supreme courts—including the New Jersey Supreme Court (on which Justice William Brennan previously served), but not the New Hampshire Supreme Court (on which Justice David Souter previously served)—have almost complete discretion over what cases they hear. The U.S. Court of Appeals, on which most of the Rehnquist Court justices previously served, does not.

DATA AND METHODS

To test the nonconsensual behavior hypothesis, we assembled information about the voting and opinion writing of the Rehnquist Court justices. The data include the dissenting votes and the concurring and dissenting opinions of the individual justices at the Supreme Court level and at the lower appellate court level. Every November edition of the *Harvard Law Review* contains statistics describing the behavior of Supreme Court justices for the most recently completed term. We used these, as well as supplemental information we gathered from the LEXIS and WESTLAW computer databases, the U.S. Supreme Court Judicial Database (Spaeth 1993), and, for the 1992–94 Supreme Court terms, *United States Law Week*. Information about the justices' lower appellate court experience was obtained via LEXIS and WESTLAW as well as various federal and state reporter series and biographical collections.[9]

To measure the degree of nonconsensual behavior exhibited by the Rehnquist Court justices, we divided the number of dissenting votes and the number of concurring and dissenting opinions they each issued, separately, by the total number of cases in which they individually participated. We refer to the outcomes of these divisions as the "nonconsensual voting ratios" and "nonconsensual opinion-writing ratios" of the individual justices at the Supreme Court level and at the lower appellate court level. The nonconsensual ratios show how much individual justices are willing to be nonconsensual from the majority by casting dissenting votes or by writing concurring or dissenting opinions.

Given that our measures of nonconsensual behavior are in the forms of ratios, we performed a series of t-tests to discern whether the justices' lower appellate court behavior correlates with their Supreme Court behavior in terms of nonconsensual opinion writing and voting. The t-tests compared the Rehnquist Court justices' Supreme Court nonconsensual ratios with their lower appellate court nonconsensual ratios.

FINDINGS

Table 1 reports the nonconsensual ratios the Rehnquist Court justices each generated as Supreme Court justices and as lower appellate court judges. It is apparent that justices become less consensual after they come to the Supreme Court. Virtually all justices show significantly different nonconsensual behavior in voting and opinion writing after they reach the Marble Temple: They dissent more and write more concurring and dissenting opinions as Supreme Court justices than they did as lower appellate court judges. The only exceptions are found in the dissenting opinion-writing behavior of Justices Anthony Kennedy and Antonin Scalia. Their respective Supreme Court dissenting opinion ratios are not statistically different from their respective lower appellate court dissenting opinion ratios.

The differences in nonconsensual ratios when the justices' individual scores were averaged together are similar to those obtained when they were analyzed separately. In dissenting voting, the nonconsensual ratio jumps nine times, from .024 to .214, and in opinion writing the cumulative nonconsensual ratio jumps five times, from .037 to .179. These differences are statistically significant at the .05 level, as are those obtained for other cumulative differences, such as those between dissenting opinions at the Supreme Court level and those at the lower appellate court level. Another interesting result is that the nonconsensual ratios of justices at the Supreme Court level tend to be higher when those justices have had prior lower appellate court experience. This implies that justices with prior lower appellate court experience are less conciliatory than those without that experience—perhaps because of the added self-confidence that likely results from having already served as an appellate court judge.[10]

Why, then, are the Rehnquist Court justices less consensual in voting and opinion writing as Supreme Court justices than they were as lower appellate court judges? In the remainder of this article we attempt to

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[9] Most lower appellate courts, including the U.S. Court of Appeals, do not publish all their decisions (e.g., Tate 1983). Although LEXIS and WESTLAW report at least some nonpublished decisions (accompanied by a notation such as "not for publication"), we were unable to determine whether they report all. This does not pose a problem for our analysis, however, because nonunanimous decisions are almost always published (e.g., Hoffman 1981, Songer 1990). In other words, our measure is *conservative* because lower court decisions that are not published are unanimous.

[10] Students of the freshman (or acclimation) effect hypothesis will be interested to note that *all* new justices who had previously served on a lower appellate court, such as Breyer, were less consensual on the Supreme Court than they were on a lower appellate court.

TABLE 1. Nonconsensual Ratios of the Rehnquist Court Justices

			Opinions			
Justice	Court	Votes Dissenting	Concurring	Dissenting	Concurring and Dissenting	*N*
Brennan	Supreme	.333*	.080*	.133*	.213*	602
	Lower	.021	.006	.021	.027	684
Marshall	Supreme	.349*	.014*	.099*	.113*	735
	Lower	.026	.000	.026	.026	418
Blackmun	Supreme	.289*	.077*	.107*	.184*	1078
	Lower	.016	.000	.016	.016	810
Stevens	Supreme	.287*	.096*	.167*	.263*	1255
	Lower	.067	.034	.067	.101	585
O'Connor	Supreme	.168*	.081*	.073*	.154*	1254
	Lower	.000	.000	.000	.000	87
Scalia	Supreme	.163*	.142*	.099	.241*	1249
	Lower	.066	.025	.066	.091	319
Kennedy	Supreme	.092*	.078*	.046	.124*	999
	Lower	.036	.057	.034	.091	1085
Souter	Supreme	.127*	.051*	.056*	.107*	625
	Lower	.013	.008	.013	.021	1337
Thomas	Supreme	.182*	.067*	.063*	.130*	506
	Lower	.010	.020	.010	.020	99
Ginsburg	Supreme	.141*	.096*	.090*	.186*	178
	Lower	.021	.029	.018	.047	1051
Breyer	Supreme	.154*	.026*	.077*	.103*	78
	Lower	.013	.008	.013	.021	1865
All justices	Supreme	.214*	.082*	.097*	.179*	8600
	Lower	.024	.016	.021	.037	8340
Nonconsensual ratios of justices without lower appellate court experience (White, Powell, Rehnquist)						
	Supreme	.133*	.030*	.062*	.092*	2495

Note: Lower court-lower appellate court. The nonconsensual ratios of justices without lower appellate court experience are compared with the nonconsensual ratios of justices with lower appellate court experience.
* $p < .05$, two-tailed test.

answer this question by holding constant the most likely institutional explanations for the differences found in the two data sets.

Court Size

Probabilistically, the larger the number of judges on a court, the greater is the likelihood of disagreement among them and the greater the opportunities for competition. Therefore, judges on larger courts should exhibit higher levels of nonconsensual behavior than do judges on smaller courts (e.g., Hall and Brace 1989, 398). Given that the Supreme Court operates with nine members, whereas lower appellate courts typically operate in panels of three (U.S. Court of Appeals) or five or seven (state supreme courts), one might expect more nonconsensual behavior by Supreme Court justices. In order to control for the size difference, we calculated the probability of a judge dissenting in each court, assuming that all other things are equal except for court size. If a judge casts votes by flipping a coin and her or his voting patterns are uniform, then s/he dissents two times out of eight possible voting combinations in a three-person court (YYY, YYN, YNY, YNN, NYY, NYN, NNY, NNN; $P = 2/8 = .250$). Similarly, a judge dissents 186 times out of 512 possible voting combinations in a nine-person court ($P = 186/512 = .363$). (See Appendix A for more details about how the probabilities are calculated.) Based on these two probabilities, we adjusted the lower court nonconsensual ratios to control for the difference in court size. More specifically, the lower court ratios are multiplied by 1.452 (.363/.250 = 1.452) for those justices who served on lower federal appellate courts, because these courts—with the exception of occasional en banc hearings[11] (which we excluded)—operate in panels of three. For William Brennan, the multiplier is 1.058 (.363/.343 = 1.058) because his lower appellate court experience was with the New Jersey Supreme Court, which operates with seven judges.[12] For David Souter, the multiplier is 1.163 (.363/.312 = 1.163) because his lower appellate court experience was with the New Hampshire Supreme Court, which operates with five judges.[13]

Table 2 displays the justices' nonconsensual ratios at the two court levels, controlling for court size. The cross-judicial differences in both the justices' individual nonconsensual ratios and the aggregate of the justices'

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[11] "En banc" hearings are those in which the entire membership of a particular court of appeals sits.

[12] Brennan served briefly on the New Jersey intermediate court of appeals before being elevated to the New Jersey Supreme Court. We exclude Brennan's tenure on the intermediate court because of insufficient data.

[13] Souter was appointed to the U.S. Court of Appeals for the First Circuit in 1990, but he was nominated to the Supreme Court before he heard any cases there. Consequently, there are no U.S. Court of Appeals data to report for Souter.

TABLE 2. Court-Size Adjusted Nonconsensual Ratios of the Rehnquist Court Justices

			Opinions			
Justice	Court	Votes Dissenting	Concurring	Dissenting	Concurring and Dissenting	N
Brennan	Supreme	.333*	.080*	.133*	.213*	602
	Lower	.022	.006	.022	.028	684
Marshall	Supreme	.349*	.014*	.099*	.113*	735
	Lower	.038	.000	.038	.038	418
Blackmun	Supreme	.289*	.077*	.107*	.184*	1078
	Lower	.023	.000	.023	.023	810
Stevens	Supreme	.287*	.096*	.167*	.263*	1255
	Lower	.096	.049	.096	.145	585
O'Connor	Supreme	.168*	.081*	.073*	.154*	1254
	Lower	.000	.000	.000	.000	87
Scalia	Supreme	.163*	.142*	.099	.241*	1249
	Lower	.094	.036	.094	.132	319
Kennedy	Supreme	.092*	.078	.046	.124	999
	Lower	.052	.083	.049	.132	1085
Souter	Supreme	.127*	.051*	.056*	.107*	625
	Lower	.015	.009	.015	.024	1337
Thomas	Supreme	.182*	.067*	.063*	.130*	506
	Lower	.015	.026	.013	.041	99
Ginsburg	Supreme	.141*	.096*	.090*	.186*	178
	Lower	.030	.042	.026	.068	1051
Breyer	Supreme	.154*	.026*	.077*	.103*	78
	Lower	.019	.012	.019	.031	1865
All justices	Supreme	.214*	.082*	.097*	.179*	8600
	Lower	.035	.023	.030	.053	8340

Note: Lower court-lower appellate court.
* $p < .05$, two-tailed test.

individual nonconsensual ratios are virtually unchanged. In fact, the only changes are that Justice Kennedy's concurring opinion-writing behavior and his combined nonconsensual opinion-writing behavior at the Supreme Court level are not statistically different from his behavior at the lower appellate court level. This consistency in the results after controlling for the probability of dissenting in each court suggests that the higher nonconsensual ratios of the justices at the Supreme Court level are not due simply to the larger size of the Supreme Court.

Of course, multiplying the lower court ratios does not address some of the qualitative aspects of various group sizes. For instance, psychologists have demonstrated that the most difficult type of nonconsensual behavior in which to engage is to stand alone from the rest of the group (see, e.g., Ulmer 1971). Consequently, it might be that dissent in lower appellate courts—at least in three-judge U.S. Court of Appeals panels—is depressed because any variance from the majority is going to be a solo act. Yet, the seven-person and five-person status of the respective state supreme courts on which Brennan and Souter served, and a separate analysis of the en banc U.S. Court of Appeals decisions of the other members of the Rehnquist Court,[14] suggest that group psychology dynamics do not play much, if any, role in explaining the depressed rates of overt nonconsensus on the lower appellate courts, at least as far as standing alone from the majority is concerned. (We address later the related issue of different court *norms* toward dissent.)

Ideology

We also take into account the effect of the ideological composition of the lower appellate courts on which the Rehnquist Court justices specifically served. One may reasonably presume that a conservative justice who served on a liberal lower appellate court would dissent less often after s/he ascended to the conservative Rehnquist Court, while a liberal justice who served on a liberal lower appellate court would dissent more often after s/he arrived at the conservative Rehnquist Court.[15] To control for the effects of the ideological composition of the lower appellate courts, those courts

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[14] The following numbers indicate the dissenting votes and concurring and dissenting opinions of the justices in all en banc federal appellate court cases in which they individually participated. Marshall: 0, 0, 0 ($N = 3$); Blackmun: 0, 0, 0 ($N = 13$); Stevens: 3, 7, 3 ($N = 13$); Scalia: 5, 0, 4 ($N = 21$); Kennedy: 6, 0, 1 ($N = 32$); Thomas: 0, 0, 0 ($N = 7$); Ginsburg: 2, 2, 1 ($N = 8$); and Breyer: 3, 0, 3 ($N = 13$). Given that the data are fairly evenly split between those justices who were more nonconsensual on the U.S. Court of Appeals during en banc proceedings than they were during three-judge panel proceedings and those who were not, it is most likely not larger court size that explains the higher nonconsensual ratios of the justices at the Supreme Court level. Of course, the small number of en banc proceedings available in our data set and the fact that such proceedings tend to involve more complicated and/or controversial case types require that these data be interpreted with caution. We have more to say below about the case-type factor.

[15] Hensley and Smith (1995) maintain that the Rehnquist Court, while conservative, is not as conservative as some scholars allege

TABLE 3. Court-Size Adjusted Nonconsensual Ratios by Justices' Ideology, Controlling for the Ideological Composition of the Lower Appellate Court

Ideology of Lower Court	Liberal				Moderate	Conservative					
Ideology of Justices[a]	Liberal		Conservative		Liberal	Liberal			Conservative		
	Blackmun[b]	Ginsburg	Kennedy	Scalia	Stevens	Brennan	Marshall	Breyer	O'Connor	Souter[c]	Thomas
	E A	E A	E A	E A	E A	E A	E A	E A	E A	E A	E A
Dissenting Votes											
Lower court nonconsensual ratios[d]	H .02	L .03	H .05	H .09	M .10	H .02	H .04	H .02	L .00	L .02	L .02
Supreme Court nonconsensual ratios[e]	H .29	H .14	L .09	L .16	H .29	H .33	H .35	H .15	L .17	L .13	L .18
Expected and actual % increase[f]	0 1350	+ 367	− 80	− 78	+ 190	0 1550	0 775	0 650	0 H	0 550	0 800
Concurring and Dissenting Opinions											
Lower court nonconsensual ratios	H .02	L .07	H .13	H .13	M .15	H .03	H .04	H .03	L .00	L .02	L .04
Supreme Court nonconsensual ratios	H .18	H .19	L .12	L .24	H .26	H .21	H .11	H .10	L .15	L .11	L .13
Expected and actual % increase	0 800	+ 171	− −8	− 85	+ 73	0 600	0 175	0 233	0 H	0 450	0 225

Note: The Rehnquist Court is assumed to be conservative. E = Expected nonconsensual ratio, A = Actual nonconsensual ratio. For expected nonconsensual ratios: L = Low, H = High, + = Increase, − = Decrease, 0 = No change.

[a] The ideological characterization of justices at the Supreme Court level conforms to the justices' actual votes in civil liberties and economics cases, as shown by Segal, Epstein, Cameron, and Spaeth (1995).

[b] Although Blackmun began his Supreme Court tenure on the Burger Court as a conservative, he was an established liberal during the Rehnquist Court period—the subject of this study.

[c] Although Souter appears to be becoming more liberal as his tenure on the Supreme Court progresses, he is not liberal in the traditional sense of seeing the Court as an engine for social change. See Greenhouse (1995).

[d] At the lower court level, a justice's nonconsensual ratios are expected to be high if the justice is liberal and the court is conservative (e.g., Marshall) and vice versa. The ratios are expected to be low if the justice is liberal and the court is also liberal (e.g., Ginsburg) or the justice is conservative and the court is also conservative (e.g., Thomas).

[e] At the Supreme Court level, a justice's nonconsensual ratios are expected to be high if the justice is liberal, low if the justice is conservative (because the Rehnquist Court is assumed to be conservative).

[f] If a justice's expected nonconsensual ratios at the lower court level are high and those at the Supreme Court level are low, then the justice is expected to show a decrease (−) in ratios. The actual change column shows that the nonconsensual ratios of virtually all justices increased.

were divided into conservative, liberal, and moderate categories. Given that the composition of these courts sometimes changed from year to year, we identified, on a year-by-year basis, the political party configuration of the lower appellate court in question while a particular justice was serving there.[16] If the majority of the judges on the court for a specified year was Republican, a value of −1 was assigned to that year. If the majority was Democratic, a value of +1 was assigned. We then aggregated the individual years to arrive at a classification of the relevant lower appellate court as conservative, liberal, or moderate. Here, we made the standard assumption that Republican judges tend to reach more conservative decisions than do Democratic judges (e.g., Baum 1990, Goldman 1975, Lloyd 1995, Nagel 1961, Tate 1981). An example will clarify our approach.

David Souter served on the New Hampshire Supreme Court from 1983 to 1990. During 1983 and 1984, John W. King, Charles Douglas, David Brock, and William Batchelder served with him. All but King were Republicans at the time (Batchelder is now an Independent). Consequently, the majority of the court in both years was Republican, so each year was assigned a value of −1. William R. Johnson replaced Douglas in 1985, with the rest of the court remaining the same. Johnson, like Douglas, is a Republican. The composition of the court did not change in 1986: 1985 and 1986 were, therefore, each assigned the value −1. In 1987 W. Stephen Thayer replaced King, and the New Hampshire Supreme Court was comprised of Brock, Batchelder, Souter, Johnson, and Thayer until 1990, when Souter left. All are Republicans, so these four years were each assigned the value −1. Adding the eight

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(e.g., Chemerinsky 1989, Irons 1990). But see Segal, Epstein, Cameron, and Spaeth 1995.

[16] The political party affiliations of the judges who served with the various Rehnquist Court justices on the U.S. Court of Appeals were obtained from *The American Bench* (1995). The political party affiliations of the judges who served with David Souter, Sandra Day O'Connor, and William Brennan on their respective state appellate courts were graciously provided by reference librarians at the New Hampshire, Arizona, and New Jersey state libraries, respectively.

years together, we get a total of −8, which, based on the standard assumption about political party identification and voting behavior, makes the New Hampshire Supreme Court conservative during Justice Souter's tenure there.[17]

Table 3 shows that individuals become more nonconsensual on the Supreme Court regardless of the ideological composition of the lower appellate court on which they previously served. As described above, it seems reasonable to suppose that, once judges reach the conservative Rehnquist Court, liberals from liberal lower appellate courts would show higher nonconsensual ratios, conservatives from liberal lower appellate courts would show lower nonconsensual ratios, and both liberals and conservatives from conservative lower appellate courts would show no change in their nonconsensual ratios. This is not the case. The nonconsensual ratios of all the justices in each group show a significant increase at the Supreme Court level. Overall, conservative justices tend to dissent less often than liberal justices do on the conservative Rehnquist Court. But, cross-judicially, the percentage change in the justices' nonconsensual ratios does not show any clear pattern of increase. Even though conservative justices from liberal lower appellate courts tend to show a small amount of increase in their nonconsensual ratios, liberal justices from conservative lower appellate courts show a large amount of increase in their nonconsensual ratios. This finding contradicts the supposition that the increase in nonconsensual ratios is primarily the product of a difference between the ideological composition of the Supreme Court and the relevant lower courts. The implication of this finding may be that virtually all justices become more nonconsensual after they come to the Supreme Court and that the individual justices' ideology affects their nonconsensual behavior more on the Supreme Court than it does on the lower appellate courts.

Case Types

Previous studies have shown that there is a difference between the subject matter of cases in the lower appellate courts and the Supreme Court (e.g., McLauchlan 1991) as well as a difference in case types across lower appellate courts (e.g., Howard 1981, Songer 1991, Tarr and Porter 1988). For example, the U.S. Court of Appeals for the District of Columbia Circuit handles a large number of administrative appeals, while other circuits do not. It is possible, therefore, that a justice's lower court nonconsensual ratios are derived from cases that do not ordinarily reach the Supreme Court. To control for the difference in case types, we divided all Supreme Court cases for the period into criminal procedure, civil rights, civil liberties, economic activities, administrative law, and "other" types (e.g., commerce clause, Eleventh Amendment), and we did the same with a sample of 2,030 lower court cases.[18]

As Table 4 illustrates, the types of cases that reach the Supreme Court are somewhat different from those that reach the lower appellate courts. Proportionally more civil rights and civil liberties cases go to the Supreme Court, whereas proportionally more economic activities and administrative law cases are decided in the lower appellate courts. However, the justices' nonconsensual ratios at the Supreme Court level are higher in virtually all types of cases. Although the justices' Supreme Court nonconsensual ratios are slightly higher in the civil rights and civil liberties cases than in other cases, this pattern is not strong enough to explain the increase in nonconsensual ratios between the two court levels. Overall, the results suggest that judges at the two court levels decide different types of cases, but this difference is not the primary reason for the higher nonconsensual ratios generated by justices at the Supreme Court level.

There is, of course, more to the case-type variable than differences in substantive categories. Many scholars suggest that there is more conflict among justices on the Supreme Court than among judges on the lower appellate courts because the Supreme Court decides more *difficult* cases than the lower courts do (e.g., Halpern and Vines 1977, Howard 1981, O'Brien 1996). Here, the argument goes, the considerable control the Supreme Court has over its docket allows the justices to accept difficult cases and dismiss easy ones, whereas many court of appeals judges (i.e., U.S. Court of Appeals judges and supreme court judges in states without intermediate appellate courts) do not have this advantage, and therefore decide great numbers of easy cases. This view is apparently shared by at least some Supreme Court justices. Clarence Thomas (1994–95, 611), for one, states:

> the cases are routinely difficult [at the Supreme Court]. That is the big difference, in my opinion, between the Supreme Court and the Court of Appeals. At least on the Court of Appeals there were some cases that didn't seem that difficult. What I find about the Supreme Court is that, even when you think you know the answer, it is hard and, hence, my gray hairs.

Unfortunately, as Songer (1986) points out, an empirical test of this supposition is complicated by the absence of any objective way to determine which cases are "difficult" and which are "easy." Songer's suggested solution to this problem when investigating variations

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[17] This method of classifying the ideological composition of the lower appellate courts works as well for the U.S. Court of Appeals as it does for the various state appellate courts because panel assignments on the former are made randomly (see, e.g., Howard 1981, 198; U.S. Court of Appeals for the Fourth Circuit 1994, 34-2). The ideology scores for the various lower courts in our data set ranged from −8 to +40. We classified two lower courts with a score of 0 as moderate, five lower courts with scores from −3 to −8 as conservative, and four lower courts with scores from +9 to +40 as liberal.

[18] We tested a sample of the lower court data because of the large number of lower-court cases we generated (8,340). To avoid any potential acclimation effect problems, we sampled a midtenure period of each justice's lower appellate court tenure. (We used the entire tenure for O'Connor and for Thomas because it was brief.) We were able to test all the Rehnquist Court cases because the U.S. Supreme Court Judicial Database classified the cases through the 1991 term, and we were able to generate and classify the more recent terms' cases ourselves.

TABLE 4. Court-Size Adjusted Nonconsensual Ratios, by Type of Case

	Case Type						
Justice	Criminal Procedure	Civil Rights	Civil Liberties	Economic Activities	Admin. Law	All Others	Total
Brennan							
Supreme Court							
Dissenting Votes	.555*	.417*	.389*	.179*	.000	.179*	.334*
Concurring Opinions	.066*	.077*	.148*	.081*	.000	.065*	.080*
Dissenting Opinions	.219*	.154*	.194*	.041	.000	.102*	.133*
Concurring & Dissenting Opinions	.285*	.231*	.342*	.122*	.000	.167*	.213*
N	137	91	108	123	9	140	602
Lower Court							
Dissenting Votes	.020	.000	.000	.036	.000	.000	.029
Concurring Opinions	.000	.000	.000	.000	.000	.000	.000
Dissenting Opinions	.020	.000	.000	.026	.000	.000	.021
Concurring & Dissenting Opinions	.020	.000	.000	.026	.000	.000	.000
N	54	0	0	201	0	48	303
Marshall							
Supreme Court							
Dissenting Votes	.592*	.402*	.412*	.215*	.000	.181*	.350*
Concurring Opinions	.012	.000	.018	.020	.133*	.018	.016
Dissenting Opinions	.231*	.098*	.050*	.060*	.000	.047*	.099*
Concurring & Dissenting Opinions	.243*	.098*	.068*	.080*	.133*	.062*	.115*
N	169	112	119	149	15	171	735
Lower Court							
Dissenting Votes	.106	.000	.000	.000	.000	.000	.049
Concurring Opinions	.000	.000	.000	.000	.000	.000	.000
Dissenting Opinions	.064	.000	.000	.028	.000	.000	.036
Concurring & Dissenting Opinions	.064	.000	.000	.028	.000	.000	.036
N	68	5	5	160	0	48	286
Blackmun							
Supreme Court							
Dissenting Votes	.356*	.365*	.373*	.239*	.268*	.169*	.260*
Concurring Opinions	.080*	.076	.184*	.032	.024	.056*	.077*
Dissenting Opinions	.114*	.157*	.171*	.059*	.073	.048*	.108*
Concurring & Dissenting Opinions	.194*	.233*	.355*	.091*	.097*	.104*	.185*
N	250	159	158	222	41	248	1078
Lower Court							
Dissenting Votes	.029	.000	.000	.000	.000	.000	.008
Concurring Opinions	.000	.000	.000	.000	.000	.000	.000
Dissenting Opinions	.029	.000	.000	.000	.000	.000	.008
Concurring & Dissenting Opinions	.029	.000	.000	.000	.000	.000	.008
N	49	2	4	109	0	3	167
Stevens							
Supreme Court							
Dissenting Votes	.369*	.239	.367*	.304*	.207*	.179*	.288*
Concurring Opinions	.096*	.098	.169	.077	.138*	.061*	.096
Dissenting Opinions	.178	.196	.215	.177	.103*	.096*	.165*
Concurring & Dissenting Opinions	.274*	.294	.384*	.254*	.241*	.157*	.261*
N	293	184	180	260	58	280	1217
Lower Court							
Dissenting Votes	.138	.181	.153	.125	.000	.000	.036
Concurring Opinions	.039	.000	.000	.046	.000	.000	.134
Dissenting Opinions	.138	.181	.153	.125	.000	.000	.036
Concurring & Dissenting Opinions	.177	.181	.153	.171	.000	.000	.170
N	73	16	19	93	0	3	204

Note: One-sample tests are conducted if the lower court nonconsensual ratios are zero. In one-sample tests, the Supreme Court nonconsensual ratios are examined to see whether they are different from zero. Generally, it is slightly easier to reject the null hypothesis in a one-sample test than in a two-sample test.

*$p < .05$, two-tailed test.

TABLE 4. (con't.)

	Case Type						
Justice	Criminal Procedure	Civil Rights	Civil Liberties	Economic Activities	Admin. Law	All Others	Total
O'Connor							
Supreme Court							
Dissenting Votes	.181*	.191*	.205*	.186*	.086*	.125*	.170*
Concurring Opinions	.096*	.098*	.156*	.069*	.035*	.022	.080*
Dissenting Opinions	.065*	.082*	.100*	.073*	.017	.068*	.073*
Concurring & Dissenting Opinions	.161*	.180*	.256*	.142*	.052*	.090*	.153*
N	293	183	180	260	58	280	1254
Lower Court							
Dissenting Votes	.000	.000	.000	.000	.000	.000	.000
Concurring Opinions	.000	.000	.000	.000	.000	.000	.000
Dissenting Opinions	.000	.000	.000	.000	.000	.000	.000
Concurring & Dissenting Opinions	.000	.000	.000	.000	.000	.000	.000
N	15	0	2	67	0	3	87
Scalia							
Supreme Court							
Dissenting Votes	.187*	.181	.207*	.131	.155*	.136*	.167*
Concurring Opinions	.126	.126*	.167*	.173*	.155*	.125*	.143*
Dissenting Opinions	.105	.093	.133*	.077	.086	.096*	.099*
Concurring & Dissenting Opinions	.231	.219	.300*	.250*	.241*	.221*	.242*
N	294	182	174	260	58	281	1249
Lower Court							
Dissenting Votes	.000	.077	.000	.116	.030	.000	.036
Concurring Opinions	.110	.000	.000	.000	.045	.000	.054
Dissenting Opinions	.000	.077	.000	.116	.030	.000	.036
Concurring & Dissenting Opinions	.110	.077	.000	.116	.075	.000	.090
N	13	13	12	25	98	0	161
Kennedy							
Supreme Court							
Dissenting Votes	.110	.113	.144*	.056	.021	.087*	.107*
Concurring Opinions	.089	.056*	.187	.052	.042*	.041*	.078
Dissenting Opinions	.055	.063	.072*	.028	.000	.037*	.046
Concurring & Dissenting Opinions	.144	.119	.259*	.080	.042*	.078*	.124
N	237	142	139	213	48	219	999
Lower Court							
Dissenting Votes	.073	.224	.000	.045	.000	.000	.055
Concurring Opinions	.097	.000	.132	.059	.000	.000	.071
Dissenting Opinions	.073	.112	.000	.045	.000	.000	.048
Concurring & Dissenting Opinions	.160	.112	.132	.104	.000	.000	.119
N	60	13	11	98	0	2	184
Souter							
Supreme Court							
Dissenting Votes	.119	.176*	.257*	.124	.041*	.039*	.133
Concurring Opinions	.053	.011	.186*	.022	.041*	.047	.053
Dissenting Opinions	.073	.066*	.157*	.036	.000	.016	.056
Concurring & Dissenting Opinions	.126	.077*	.343*	.057	.041*	.063	.109
N	151	91	70	137	49	127	625
Lower Court							
Dissenting Votes	.076	.000	.000	.120	.000	.000	.077
Concurring Opinions	.076	.000	.000	.120	.000	.106	.092
Dissenting Opinions	.076	.000	.000	.120	.000	.000	.077
Concurring & Dissenting Opinions	.152	.000	.000	.240	.000	.106	.169
N	31	3	2	29	0	11	76

Note: One-sample tests are conducted if the lower court nonconsensual ratios are zero. In one-sample tests, the Supreme Court nonconsensual ratios are examined to see whether they are different from zero. Generally, it is slightly easier to reject the null hypothesis in a one-sample test than in a two-sample test.
$^*p < .05$, two-tailed test.

TABLE 4. (con't)

	Case Type						
Justice	Criminal Procedure	Civil Rights	Civil Liberties	Economic Activities	Admin. Law	All Others	Total
Thomas							
Supreme Court							
Dissenting Votes	.273*	.074*	.352*	.220*	.372*	.147*	.188*
Concurring Opinions	.066*	.147*	.093*	.055*	.116*	.000	.067*
Dissenting Opinions	.099*	.030	.093*	.028	.093*	.055*	.063*
Concurring & Dissenting Opinions	.165*	.177*	.186*	.083*	.209*	.055*	.130*
N	121	68	54	109	43	109	506
Lower Court							
Dissenting Votes	.000	.000	.000	.000	.000	.000	.000
Concurring Opinions	.000	.000	.000	.000	.083	.000	.030
Dissenting Opinions	.000	.000	.000	.000	.000	.000	.000
Concurring & Dissenting Opinions	.000	.000	.000	.000	.083	.000	.030
N	20	9	5	26	35	4	99
Ginsburg							
Supreme Court							
Dissenting Votes	.068*	.250	.526*	.154	.118	.057	.180*
Concurring Opinions	.159*	.083	.211*	.000	.059	.029	.079
Dissenting Opinions	.068*	.125	.263*	.077	.059	.000	.101
Concurring & Dissenting Opinions	.227*	.208	.474*	.077	.118	.029	.180*
N	44	24	19	39	17	35	178
Lower Court							
Dissenting Votes	.000	.242	.000	.035	.045	.000	.048
Concurring Opinions	.000	.159	.000	.035	.031	.000	.039
Dissenting Opinions	.000	.242	.000	.035	.031	.000	.048
Concurring & Dissenting Opinions	.000	.401	.000	.070	.062	.000	.087
N	14	18	15	42	96	13	198
Breyer							
Supreme Court							
Dissenting Votes	.111	.462*	.300	.063*	.000	.143	.180*
Concurring Opinions	.000	.000	.000	.000	.000	.071	.039
Dissenting Opinions	.111	.154*	.100	.000	.000	.143	.077*
Concurring & Dissenting Opinions	.111	.154	.100	.000	.000	.214*	.116*
N	18	13	10	16	7	14	78
Lower Court							
Dissenting Votes	.074	.000	.322	.000	.000	.000	.022
Concurring Opinions	.000	.045	.000	.011	.000	.000	.012
Dissenting Opinions	.074	.000	.322	.000	.000	.000	.022
Concurring & Dissenting Opinions	.074	.045	.322	.011	.000	.000	.034
N	78	32	9	120	0	26	265
All justices							
Supreme Court							
Dissenting Votes	.280*	.242*	.302*	.176*	.129*	.140*	.218*
Concurring Opinions	.085*	.079	.151*	.070*	.079	.052*	.082*
Dissenting Opinions	.122*	.112	.136*	.073*	.050	.065*	.096*
Concurring & Dissenting Opinions	.207*	.191	.287*	.143*	.129*	.117*	.178*
N	2008 (23%)	1252 (15%)	1203 (14%)	1788 (21%)	403 (5%)	1946 (22%)	8600 (100%)
Lower Court							
Dissenting Votes	.073	.079	.138	.037	.012	.018	.048
Concurring Opinions	.046	.052	.017	.018	.044	.018	.030
Dissenting Opinions	.049	.104	.069	.040	.025	.027	.044
Concurring & Dissenting Opinions	.095	.157	.086	.058	.069	.045	.074
N	475 (23%)	111 (6%)	84 (4%)	970 (48%)	229 (11%)	161 (8%)	2030 (100%)

Note: One-sample tests are conducted if the lower court nonconsensual ratios are zero. In one-sample tests, the Supreme Court nonconsensual ratios are examined to see whether they are different from zero. Generally, it is slightly easier to reject the null hypothesis in a one-sample test than in a two-sample test.

*$p < .05$, two-tailed test.

TABLE 5. Court-Size Adjusted Nonconsensual Ratios in Difficult Cases

			Opinions			
Justice	Court	Votes Dissenting	Concurring	Dissenting	Concurring and Dissenting	*N*
Lower Court Cases in which Supreme Court Review Was Sought						
Brennan	Supreme	.333*	.080*	.133	.213*	602
	Lower	.086	.000	.057	.057	35
Marshall	Supreme	.349*	.014	.099	.113*	735
	Lower	.050	.008	.050	.058	122
Blackmun	Supreme	.289*	.077*	.107*	.184*	1078
	Lower	.013	.000	.013	.013	77
Stevens	Supreme	.287*	.096*	.167*	.263*	1255
	Lower	.091	.008	.083	.091	121
O'Connor	Supreme	.168*	.081*	.073*	.154*	1254
	Lower	.000	.000	.000	.000	0
Scalia	Supreme	.163*	.142*	.099	.241*	1249
	Lower	.120	.020	.120	.140	50
Kennedy	Supreme	.092*	.078	.046	.124*	999
	Lower	.048	.081	.032	.113	62
Souter	Supreme	.127*	.051*	.056*	.107*	625
	Lower	.000	.000	.000	.000	8
Thomas	Supreme	.182*	.067*	.063*	.130*	506
	Lower	.000	.000	.000	.000	13
Ginsburg	Supreme	.141*	.096	.090	.186*	178
	Lower	.063	.041	.063	.104	48
Breyer	Supreme	.154*	.026	.077	.103*	78
	Lower	.025	.000	.025	.025	80
All justices	Supreme	.214*	.082*	.097*	.179*	8600
	Lower	.072	.016	.067	.083	617
Lower Court Cases in which Supreme Court Review Was Not Sought						
Brennan	Supreme	.333*	.080*	.133*	.213*	602
	Lower	.032	.004	.024	.028	509
Marshall	Supreme	.349*	.014	.099*	.113*	735
	Lower	.017	.017	.017	.034	295
Blackmun	Supreme	.289*	.077*	.107*	.184*	1078
	Lower	.008	.004	.008	.012	244
Stevens	Supreme	.287*	.096*	.167*	.263*	1255
	Lower	.043	.033	.043	.076	301
O'Connor	Supreme	.168*	.081*	.073*	.154*	1254
	Lower	.000	.000	.000	.000	87
Scalia	Supreme	.163*	.142*	.099*	.241*	1249
	Lower	.041	.025	.041	.066	242
Kennedy	Supreme	.092*	.078	.046	.124*	999
	Lower	.008	.031	.008	.039	262
Souter	Supreme	.127*	.051*	.056*	.107*	625
	Lower	.015	.010	.015	.025	606
Thomas	Supreme	.182*	.067*	.063*	.130*	506
	Lower	.023	.012	.023	.035	86
Ginsburg	Supreme	.141*	.096	.090	.186*	178
	Lower	.016	.028	.016	.044	247
Breyer	Supreme	.154*	.026	.077*	.103*	78
	Lower	.012	.014	.012	.026	485
All justices	Supreme	.214*	.082*	.097*	.179*	8600
	Lower	.044	.016	.020	.036	3291

Note: All Supreme Court cases are assumed to be difficult.

* $p < .05$, two-tailed test; one-sample tests are conducted if nonconsensual ratios are zero.

in rates of dissent in the U.S. Court of Appeals was to examine several categories of cases that "might reasonably be suspected" of containing a greater proportion of cases with difficult legal questions than other cases. Although Songer was engaged in a horizontal comparison of dissent rates in various U.S. Courts of Appeals, he identified a category of cases that also can well serve our cross-judicial (i.e., vertical) analysis, namely, lower appellate court cases for which Supreme Court review was sought. The reasons Songer's method is excellent for objectifying the "difficult" case-type variable are because (1) appellant attorneys seem more likely to seek Supreme Court review if the appeals court decision turned on a difficult question of law, and (2) the Supreme Court is more likely to accept for review those cases presenting difficult questions of law (see

Songer 1986, 121–3). The data to test this are presented in Table 5.[19]

Table 5 reports that at the lower appellate court level the justices cast more dissenting votes and wrote more concurring and dissenting opinions in difficult cases than in easy cases. As a consequence, increases in the justices' nonconsensual ratios are generally lower in the cases for which Supreme Court review was sought. However, most of the justices' Supreme Court nonconsensual ratios are significantly higher than their lower court nonconsensual ratios in both easy and difficult cases. In fact, the statistical test results in both types of cases are virtually the same. Therefore, it is clear that the fact that Supreme Court justices deal with more difficult cases than lower appellate court judges do is not the primary reason for the increases in the justices' nonconsensual ratios.

Stare Decisis

The conventional wisdom is that, because the U.S. Supreme Court is the apex of the U.S. judicial system, Supreme Court justices are freer to overturn precedents than are lower court judges (see, e.g., Brenner and Spaeth 1995). Indeed, some scholars suggest that stare decisis is largely responsible for the high levels of consensus on lower appellate courts (see, e.g., Goldman and Jahnige 1985, 165). Unfortunately, as Songer (1986, 124) succinctly observes, "it would appear to be impossible to test the application of this hypothesis to the courts of appeals with the methods scholars have applied to the Supreme Court. Judges of the courts of appeals will rarely admit to overturning Supreme Court precedent directly."

This may be true when testing for variations in dissent rates across the *same* lower court level (e.g., the U.S. Court of Appeals), but it is possible to test for the effects of stare decisis when engaging in *cross*-judicial analysis, namely, by examining the cases in which judges were faced with the question of overturning *their own* precedent. After all, cross-judicial analysis is concerned with the behavioral differences, if any, exhibited by the *same* individuals at *different* court levels. With respect to stare decisis itself, the supposition is that because an individual may feel more freedom to overturn a precedent as a Supreme Court justice than s/he did as a lower appellate court judge, s/he will exhibit greater nonconsensual behavior when deciding cases in which overruling a precedent is an issue on the Supreme Court than s/he did on a lower appellate court.[20] If this supposition is correct, the increases in the justices' nonconsensual ratios between the two court levels should be more conspicuous in cases where overruling a precedent—especially a constitutional precedent[21]—is an issue and the justices should have higher Supreme Court nonconsensual ratios in this type of case.

As Table 6 shows, the justices were involved in more cases where overruling a precedent was an issue at the Supreme Court level than they were at the lower appellate court level. However, their Supreme Court nonconsensual ratios were somewhat lower in the overruling cases than in the nonoverruling cases, whereas their lower court nonconsensual ratios were somewhat higher in the overruling cases than in the nonoverruling cases. Furthermore, the cross-judicial differences in the justices' nonconsensual ratios in the overruling cases are less than those in the nonoverruling cases. The increases in the justices' aggregate nonconsensual ratios in the overruling cases range from 1.43 (.067 to .096) to 1.89 (.056 to .106) times, compared to 2.28 (.036 to .082) to 4.80 (.044 to .211) times in the nonoverruling cases (see rows for all justices). As a result, fewer of the cross-judicial differences in the nonconsensual ratios are statistically significant in the overruling cases. This is partly due to the smaller number of cases in which overruling a precedent is an issue, but it also indicates that the ratio differences cannot be explained by the fact that Supreme Court justices feel less constrained about overruling precedents than lower appellate court judges do. It should be noted here that the vast majority of cases do not deal with overturning precedents and justices show significant increases in their respective nonconsensual ratios in the nonoverruling cases (Justice Kennedy is an exception).

When we subdivide the overruling cases into cases in which overruling a constitutional interpretation precedent was an issue and cases in which overruling a statutory interpretation precedent was an issue, the number of cases for each justice becomes too small to do any statistical test. However, the aggregate numbers (for all justices) may shed some light on whether justices show greater nonconsensual behavior as Supreme Court justices when overruling a constitutional precedent is an issue than they do when overruling a statutory precedent is an issue, and it also may suggest whether the increases in the nonconsensual ratios between the two court levels are more conspicuous in the constitutional type of case.

In cases where overruling a constitutional interpretation precedent was an issue, the nonconsensual ratios (dissenting vote, concurring opinion, dissenting opin-

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[19] The data consist of all cases decided by the individual justices during a four-year, midtenure period of their respective lower appellate court tenures. Songer breaks down the difficult cases into categories where Supreme Court review was sought but denied and where it was sought and granted. We combined the categories, both because the data are easier to present when we do and because the two categories are subsets of the larger category, lower court cases in which Supreme Court review was sought.

[20] Brenner and Spaeth point out that the empirical studies of stare decisis fail to agree on what cases to examine (compare Banks 1992, Brenner and Spaeth 1995, Danelski 1986, Schmidhauser 1962, Ulmer 1959). We use those cases in which at least one member of the court addressed the issue of overruling a precedent. Although the issue may be more dramatically presented when the court actually overrules a precedent (Brenner and Spaeth 1995, 72–3), we use the cases we do in order to have adequate lower court data to analyze.

[21] Brenner and Spaeth (1995) analyze in their book-length treatment of stare decisis several issues that either are irrelevant to our cross-judicial analysis (e.g., the age of the overruled precedent) or would make our analysis unnecessarily complex (e.g., breaking down the bases on which precedent is overruled beyond the constitutional and statutory law dichotomy).

TABLE 6. Court-Size Adjusted Nonconsensual Ratios in Overruling and Nonoverruling Cases

			Opinions			
Justice	Court	Votes Dissenting	Concurring	Dissenting	Concurring and Dissenting	*N*
Overruling Cases[a]						
Brennan	Supreme	.533*	.000	.267*	.267*	15
	Lower	.151	.000	.000	.000	7
Marshall	Supreme	.550*	.000	.150	.150	20
	Lower	.000	.000	.000	.000	5
Blackmun	Supreme	.125	.063	.125	.188*	16
	Lower	.000	.000	.000	.000	7
Stevens	Supreme	.222*	.111	.148	.259*	27
	Lower	.000	.150	.000	.150	7
O'Connor	Supreme	.111	.185*	.037	.222*	27
	Lower	.000	.000	.000	.000	0
Scalia	Supreme	.074	.074	.074	.148	27
	Lower	.000	.000	.132	.132	8
Kennedy	Supreme	.000	.000	.000	.000	27
	Lower	.062	.000	.000	.000	17
Souter	Supreme	.111	.000	.111	.111	27
	Lower	.000	.000	.194	.194	12
Thomas	Supreme	.074	.185*	.111	.296	27
	Lower	.000	.000	.352	.352	3
Ginsburg	Supreme	.052	.000	.210	.210	19
	Lower	.212	.071	.141	.212	15
Breyer	Supreme	.273*	.000	.091	.091	11
	Lower	.000	.075	.075	.150	14
All justices	Supreme	.106	.072	.096	.168	208
	Lower	.056	.034	.067	.101	95
Nonoverruling Cases						
Brennan	Supreme	.342*	.080*	.131*	.211*	587
	Lower	.022	.000	.022	.022	677
Marshall	Supreme	.348*	.014	.098*	.112*	715
	Lower	.057	.000	.057	.057	413
Blackmun	Supreme	.290*	.075*	.108*	.183*	1049
	Lower	.028	.000	.028	.028	803
Stevens	Supreme	.241*	.094*	.165*	.259*	1214
	Lower	.144	.065	.144	.209	578
O'Connor	Supreme	.168*	.081*	.071*	.152*	1224
	Lower	.000	.000	.000	.000	87
Scalia	Supreme	.162*	.140*	.097*	.237*	1212
	Lower	.015	.057	.015	.072	311
Kennedy	Supreme	.091*	.080[b]	.046	.126[b]	967
	Lower	.076	.122	.076	.198	1068
Souter	Supreme	.122*	.048	.055	.103*	606
	Lower	.021	.010	.021	.031	1325
Thomas	Supreme	.179*	.067	.065*	.132*	491
	Lower	.015	.030	.015	.045	96
Ginsburg	Supreme	.124*	.100*	.088*	.188*	170
	Lower	.039	.059	.033	.092	1036
Breyer	Supreme	.153*	.028*	.083*	.111*	72
	Lower	.018	.011	.018	.029	1851
All justices	Supreme	.211*	.082*	.096*	.178*	8348
	Lower	.044	.036	.029	.071	8245

[a] Cases in which overruling a precedent was an issue.

[b] The nonconsensual ratio at the lower court is higher.

* $p < .05$, two-tailed test; one-sample tests are conducted if nonconsensual ratios are zero.

ion, and concurring and dissenting opinion) were .102, .056, .102, and .158 at the Supreme Court level (N = 108) and .046, .000, .046, and .046 at the lower appellate court level (N = 24). In cases where overruling a statutory interpretation precedent was an issue, the nonconsensual ratios were .110, .070, .100, and .170 at the Supreme Court level (N = 100) and .056, .042, .085, and .127 at the lower appellate court level (N = 71). Although the justices, perhaps surprisingly, exhibit somewhat greater nonconsensual behavior on the Supreme Court (and on the lower courts, too) in the statutory category of cases than they do in the constitutional category of cases, the differences in the ratios between the two court levels are not statistically signif-

TABLE 7. Justices' Nonconsensual Ratios and Standardized Nonconsensual Scores at the Supreme Court Level and at the Lower Court Level

		Votes		Opinions						
Justice	Court	Dissenting	Standardized Scores	Concurring	Standardized Scores	Dissenting	Standardized Scores	Concurring and Dissenting	Standardized Scores	*N*
Brennan	Supreme	.333	.914*	.080	.634*	.133	.007*	.213	.702*	602
Mean	Supreme	.240	0	.060	0	.130	0	.170	0	5184
Brennan	Lower	.068	−.854	.006	.017	.022	−.360	.028	−.366	177
Mean	Lower	.120	0	.006	0	.023	0	.029	0	1239
Marshall	Supreme	.349	1.156*	.014	−1.909[a]	.099	−.168	.113	−.916[a]	735
Mean	Supreme	.240	0	.060	0	.110	0	.170	0	6264
Marshall	Lower	.031	−.058	.000	−1.222	.031	−.058	.031	−.578	130
Mean	Lower	.032	0	.018	0	.032	0	.032	0	1022
Blackmun	Supreme	.289	.809*	.077	.132*	.107	.183*	.184	.213*	1078
Mean	Supreme	.210	0	.070	0	.100	0	.170	0	14292
Blackmun	Lower	.000	−.687	.000	−.377	.000	−.687	.000	−.751	82
Mean	Lower	.007	0	.002	0	.007	0	.009	0	543
Stevens	Supreme	.287	.886[a]	.096	.645	.167	2.128*	.263	1.743*	1255
Mean	Supreme	.210	0	.070	0	.090	0	.170	0	15075
Stevens	Lower	.093	1.458	.028	.588	.093	1.458	.121	1.448	108
Mean	Lower	.043	0	.017	0	.043	0	.060	0	815
O'Connor	Supreme	.168	−.444[a]	.081	.216*	.073	−.532*	.154	−.201*	1254
Mean	Supreme	.210	0	.070	0	.090	0	.170	0	15075
O'Connor	Lower	.000	−.842	.000	−.333	.000	−.842	.000	−.835	43
Mean	Lower	.018	0	.002	0	.018	0	.020	0	558
Scalia	Supreme	.163	−.499[a]	.142	1.964*	.099	.203	.241	1.350*	1249
Mean	Supreme	.210	0	.070	0	.090	0	.170	0	15075
Scalia	Lower	.065	.293	.039	.295	.065	.293	.104	.329	77
Mean	Lower	.050	0	.034	0	.050	0	.084	0	806
Kennedy	Supreme	.092	−1.298[a]	.078	.509[a]	.046	−1.192[a]	.124	−.582[a]	999
Mean	Supreme	.200	0	.060	0	.090	0	.160	0	12429
Kennedy	Lower	.025	−.216	.063	2.528	.025	−.216	.088	1.235	80
Mean	Lower	.027	0	.015	0	.027	0	.042	0	2084
Souter	Supreme	.127	−.798[a]	.051	−.579[a]	.056	−.895[a]	.107	−1.173[a]	625
Mean	Supreme	.200	0	.070	0	.090	0	.170	0	4707
Souter	Lower	.017	.682	.028	1.394	.017	.682	.045	1.232	172
Mean	Lower	.015	0	.012	0	.015	0	.027	0	860
Thomas	Supreme	.182	.022*	.067	−.617*	.063	−.684	.130	−.926*	506
Mean	Supreme	.180	0	.090	0	.090	0	.180	0	3627
Thomas	Lower	.000	−.774	.000	−1.049	.000	−.774	.000	−1.205	31
Mean	Lower	.031	0	.027	0	.031	0	.058	0	848
Ginsburg	Supreme	.141	−.621	.096	−.260	.090	−.144*	.186	−.325*	178
Mean	Supreme	.180	0	.110	0	.100	0	.210	0	1557
Ginsburg	Lower	.014	−.774	.024	−.296	.014	−.774	.038	−.706	71
Mean	Lower	.050	0	.034	0	.050	0	.084	0	806
Breyer	Supreme	.154	−.743[a]	.026	−.829	.077	−.514[a]	.103	−.893[a]	78
Mean	Supreme	.200	0	.110	0	.100	0	.210	0	774
Breyer	Lower	.016	−.006	.000	−.913	.016	−.006	.016	−.352	124
Mean	Lower	.018	0	.008	0	.018	0	.019	0	927

Note: Whenever possible, a midtenure one-year sample of each justice's lower court tenure is used.

[a] The standardized nonconsensual score is higher at the lower court.

* $p < .05$, two-tailed test.

icant at the .05 level in either category of cases, confirming the results in Table 6.

Norms

Our cross-judicial empirical analysis of the Rehnquist Court justices would not be complete without taking into account individual court norms toward nonconsensus. Concisely stated, the issuance of dissenting votes and concurring and dissenting opinions may be a more acceptable form of behavior on the Supreme Court than it is on (some) lower appellate courts, and this may explain (some of) the justices' higher nonconsensual ratios at the Supreme Court level. Howard (1981) demonstrated that there were different norms about dissent on the U.S. Courts of Appeals for the Second, Fifth, and D.C. Circuits during the period of his study (1960–79) and Walker, Epstein, and Dixon (1988) chronicled how the leadership style introduced by Harlan Fiske Stone in 1941 had a "dramatic" effect on the consensus norms of the Supreme Court. In a cross-judicial study such as ours, the question that needs to be addressed is whether a judge (later, justice) who exhibits low (or high) levels of nonconsensus on a particular lower court, *relative to the other judges on that court*, exhibits similarly low (or high) levels of nonconsensus on the Supreme Court, *relative to the other justices on the Supreme Court*. The data to test this are presented in Table 7.

Table 7 displays the justices' nonconsensual ratios relative to the mean nonconsensual ratios of the lower courts on which they respectively served and the justices' nonconsensual ratios relative to the mean nonconsensual ratios of the Supreme Court during their respective tenures there. The mean of the aggregated individual nonconsensual ratios of the particular court under consideration (e.g., D.C. Circuit, U.S. Supreme Court) may be construed as the norm about nonconsensus on that particular court during the period a particular justice served there.[22]

Each of the lower courts' mean ratios in all four categories of nonconsensual behavior is different (Chi-square test is significant at the .05 level), indicating that the lower courts have different norms regarding nonconsensual behavior by judges. However, the results are mixed on whether justices exhibit a consistent pattern of relative nonconsensual behavior as they move from a lower court to the Supreme Court. Of the eleven justices considered in this study, Stevens, Scalia, Thomas, Ginsburg, and Breyer show a pattern of consistent relative nonconsensual behavior: They either had above-average nonconsensual ratios on their respective lower courts and above-average nonconsensual ratios on the Supreme Court (Stevens and Scalia), or below-average nonconsensual ratios on their respective lower courts and below-average nonconsensual ratios on the Supreme Court (Thomas, Ginsburg, and Breyer). But, Blackmun and Souter show a clear pattern of inconsistent relative nonconsensual behavior: They either had below-average nonconsensual ratios on their respective lower courts and above-average nonconsensual ratios on the Supreme Court (Blackmun), or above-average nonconsensual ratios on their respective lower courts and below-average nonconsensual ratios on the Supreme Court (Souter).

When the justices' nonconsensual ratios are standardized to account for the differences in court norms (by subtracting the mean nonconsensual ratios of their respective courts from the justices' individual nonconsensual ratios and dividing the outcome by the standard deviations of their respective courts), similar results are obtained. The standardized Supreme Court nonconsensual scores (relative nonconsensual ratios) of Brennan, Blackmun, Stevens, O'Connor, and Thomas are mostly higher than their standardized lower court nonconsensual scores, whereas the standardized Supreme Court nonconsensual scores of Kennedy, Souter, and Breyer are mostly lower than their standardized lower court nonconsensual scores. Overall, out of the total of forty-four t-tests performed on the cross-judicial differences in the justices' relative nonconsensual ratios, twenty-one show a significant increase and sixteen show a significant decrease. These results suggest that the cross-judicial differences in the justices' relative nonconsensual ratios do not show any clear pattern. In other words, the justices' relative nonconsensual ratios do not all go up or go down as they move from the lower appellate court level to the Supreme Court level. Instead, the differences vary by individual justices. Consequently, it appears that the differences in lower court norms regarding nonconsensual behavior do not tell us much, presumably because the differences in norms *between* the lower courts and the Supreme Court are much greater than the differences in norms *among* the lower courts (more on this in the conclusion).

DISCUSSION AND CONCLUSIONS

Chief Justice Marshall was determined to make the Supreme Court a force in U.S. politics. In fact, he discouraged nonconsensual voting and opinion-writing behavior by the justices because he believed it would make the Court look weak and insignificant. Many of today's leading public law specialists also express concern about nonconsensual Supreme Court behavior. O'Brien expresses the concern succinctly in his prize-winning book on the Court. "Less agreement and more numerous and longer opinions," he writes, "invite uncertainty and confusion about the Court's rulings, interpretation of law, and policy-making" (1996, 336). And, as Justice White (1993) emphasized upon his retirement, this uncertainty and confusion about what the Supreme Court is saying—or better yet, about what the Court is *trying* to say—is not without practical significance for American politics: It adversely affects implementation of and compliance with Supreme Court decisions (see, generally, Johnson and Canon 1984). Put simply, the argument goes, how can the lower courts—let alone Congress, the president, the states, and the American people—implement and comply with Supreme Court decisions if they do not understand what they are?

Of course, not everyone views nonconsensual voting and opinion-writing behavior by Supreme Court justices in negative terms. The most recent statement in favor of such behavior was offered by Justice Scalia in the 1994 annual lecture of the Supreme Court Historical Society. Scalia maintains that separate opinions and votes "augment rather than diminish the prestige of the Court." In his judgment, when artificial unanimity is achieved by suppressing disagreement, the effect is to "deprive genuine unanimity of the great force it can have when that force is most needed." To illustrate his point, Scalia invokes the landmark school desegregation case, *Brown* v. *Board of Education* (1954), which had great impact in large part because it was unanimous. "Would [*Brown*] have had that effect if *all* the decisions of the Supreme Court . . . were announced as unanimous? Surely not" (Scalia 1994, 35).

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[22] Using the mean of the aggregated individual nonconsensus ratios for the different courts' norms about overt nonconsensus permits us to subsume into our discussion about norms the related question of the impact different courts' differing decision-making procedures has on nonconsensus levels (see, generally, Atkins and Green 1976; Howard 1981, 194).

Scalia offers additional reasons for valuing nonconsensual behavior by Supreme Court justices. One is that when the Court makes "a truly horrendous mistake, it is comforting—and conducive of respect for the Court—to look back and realize that at least some of the justices saw the danger clearly, and gave voice, often eloquent voice, to their concern" (1994, 35). Other reasons include that separate opinions have "kept the Court in the forefront of the intellectual development of the law" and that draft separate opinions may "improve the majority opinion"—either by encouraging the author of the majority opinion to refine her or his arguments or by *becoming* the majority opinion (pp. 39, 41–2).

Whatever one's position about the value of nonconsensual opinion writing and voting behavior by Supreme Court justices may be, our cross-judicial analysis of the Rehnquist Court justices suggests that it is extremely difficult to predict whether a given nominee to the Supreme Court will be a consensus-builder on that court. As this study has shown, virtually all justices become less consensus prone on the Supreme Court than they were on a lower appellate court, and many justices who generated relatively low nonconsensual ratios on a lower appellate court generated relatively high nonconsensual ratios on the Supreme Court. Consequently, a search such as President Clinton's for a nonpartisan consensus-builder for the Supreme Court is quixotic, at least when the search is focused on individuals with lower appellate court experience.[23]

Why, then, did the Rehnquist Court justices cast significantly more dissenting votes and issue significantly more concurring and dissenting opinions as Supreme Court justices than they did as lower appellate court judges? Given that we controlled for the most important institutional differences between the two appellate court levels, perhaps it has something to do with role conceptions. In other words, perhaps the justices believe that Supreme Court justices should and can determine policy for the nation, whereas lower court judges should not and cannot. After all, as Gibson argues, "judges' decisions are a function of what they prefer to do, tempered by what they think they ought to do, but constrained by what they feel is feasible to do" (Gibson 1983, 32; see, generally, Gibson 1981, North 1990). Consequently, once an individual is elevated to the Supreme Court, s/he may believe it is desirable, necessary, and possible to express policy disagreements with the majority via separate opinions and votes. Justice Brennan emphasized the unique role of Supreme Court justices in a widely read law review article on appellate courts:

> One enters a new and wholly unfamiliar world when he joins the Supreme Court of the United States, and this is as true of a Justice who comes from a federal court of appeals as it is of a Justice, like me, who came from a state supreme court. I say categorically that no prior experience, including prior judicial experience, prepares one for the work of the Supreme Court. I have sat with six colleagues appointed from federal courts of appeals and can confirm from my own experience how very right Mr. Justice Frankfurter was when he said "even Justices who have come to the Court from a longish and conspicuously competent tenure on the lower federal courts do not find the demands of the new task familiar." The initial confrontation on the United States Supreme Court with the astounding differences in function and character of role, and the necessity for learning entirely new criteria for decisions, can be a traumatic experience for the neophyte (Brennan 1973, 484; see also O'Connor 1981, Stevens 1982).

At the end of the day, then, perhaps what our study suggests is that, as political scientists have described in detail elsewhere (e.g., Segal and Spaeth 1993), what is most important for anticipating the behavior of Supreme Court justices is information about their policy preferences,[24] since consensus in and of itself is not viewed by the justices, except on the rarest occasions (e.g., transformative moments like school desegregation) as something for which to strive. Statements like those offered by the Clinton administration about a given nominee's "genius for consensus-building" are therefore unhelpful, because they distract our attention—and that of the Senate Judiciary Committee—from what should be the fundamental concern during the confirmation process: the nominee's policy goals. Indeed, although Justice Brennan had a reputation for being a consensus-builder on the Supreme Court, what he really attempted to build were *coalitions* for *liberal* policies (e.g., Friedelbaum 1991, Greenhouse 1990). Tables 1–7 show that Brennan was one of the least consensual members of the conservative Rehnquist Court in terms of his voting and opinion-writing behavior. He was not, in other words, acting in the *nonpartisan* manner in which the Clinton administration attempted to portray then Judge Breyer's anticipated Supreme Court behavior. Once again we see that, at bottom, the Supreme Court is a unique institution and what matters, above all else, are the policy goals of the individual justices.

That said, our *empirically* based conclusions should be supplemented in the future by *qualitative* techniques such as interviews of the Rehnquist Court justices *and* the respective lower appellate court judges with whom they previously served; questionnaires completed by the justices and judges; and interpretations of the justices' and judges' private papers when, if ever, those materials become widely available.[25] In the meantime, scholars could apply our cross-judicial perspective to earlier Supreme Court periods (e.g., Burger Court, Warren Court, Vinson Court) to see whether they can verify our supposition that role conceptions are the principal reason why individuals become more nonconsensual in voting and opinion-writing behavior on the Supreme Court than they were on a lower appellate court, or they could create a more complex model than we have employed here in the hopes of ascertaining the relative impact of various influences on the justices' nonconsensual voting and opinion-writing behavior at the two court levels. Scholars also could apply cross-judicial methodology to substantive questions other than those relating directly to consensus/conflict on appellate courts. In short, we hope that this article marks the beginning of more widespread use of cross-judicial empirical analysis, given the potential this method of analysis has for demonstrating the importance of institutional constraints on judicial behavior. After all, it is difficult to imagine a better public law test of the neoinstitutional perspective than a "natural experiment" that compares the behavior of the *same* individuals at *different* court levels.

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[23] Of course, a judge could have very low nonconsensus scores on a lower appellate court and still not be a consensus-builder there. In other words, building consensus involves more than going along with the majority: It also involves possessing and exercising the leadership skills necessary to convince colleagues to do the same. This further demonstrates the difficulty of predicting whether a potential Supreme Court nominee will be a consensus-builder on the Supreme Court.

[24] Although Segal and Spaeth (1993) may go too far when they argue that policy goals are the *only* goals shaping the behavior of Supreme Court justices, most public law specialists believe that policy goals are the *primary* determinants of the justices' positions (e.g., Baum 1994, George and Epstein 1992).

[25] Of those members of the Rehnquist Court who had previously served on a lower appellate court, only Brennan and Marshall have

APPENDIX A

If each judge's votes are uniformly distributed between "yes" and "no," and s/he casts votes by flipping a coin, the following voting combinations are possible in a three-person court.

1 YYY
2 YYN
3 YNY
4 YNN
5 NYY
6 NYN
7 NNY
8 NNN

Each column represents a judge, each row a possible combination of votes. The first judge dissents in cases 4 and 5, the second in cases 3 and 6, and the third in cases 2 and 7. The probability of dissenting for each judge is 2/8 = .250. Notice that each judge's votes are equally divided between "yes" and "no," the probability of dissenting is the same for all judges, and the second half of the cases (5 to 8) is the mirror image of the first half (1 to 4).

If the number of judges on a court increases, it is very cumbersome to show their voting combinations in this way. Therefore, a formula is developed to calculate the probability of dissenting. Suppose that a court has $2N + 1$ members (when $N = 1$, court size = 3). The votes of the members can be represented by a string of $2N + 1$ choices of one ("yes") or zero ("no"). Judge A is a member of this court and votes 1. (Here we deal with only the probability that Judge A votes 1, but the probability that s/he votes 0 is the same, as shown above.) What is the probability that A dissents? If A votes 1, then s/he dissents if the number of ones in the remaining $2N$ positions is less than or equal to $N - 1$. In that case the number of ones is at most N (including A's vote), and there are at least $N + 1$ zeros. In other words, A dissents if the sum of the binary digits (ones and zeros) is less than or equal to $N - 1$. For example, if A votes 1 in a three-person court, s/he dissents if the number of ones from the remaining judges is less than or equal to $N - 1$ (0). Note that there are a total of 2^{2N} possible voting combinations in a court with $2N + 1$ members (4 in a three-person court because we deal with only ones, not zeros). Therefore, the probability that A dissents is:

$\frac{1}{2^{2N}}$ (the number of strings of $2N$ ones and zeros, the sum of whose digits is no more than $N-1$).

The number of ways to add up $2N$ ones or zeros and get exactly k is "$2N$ things taken k at a time."

That is, $\binom{2N}{k}$.

The number of ways to add up to at most $N - 1$ is:

$$\sum_{k=0}^{N-1}\binom{2N}{k}.$$

Therefore, the probability that A dissents on a court with $2N + 1$ members is:

$$\frac{1}{2^{2N}}\sum_{k=0}^{N-1}\binom{2N}{k}.$$

The denominator, 2^{2N}, is the number of total voting combinations, and the numerator is the number of possible voting combinations in which A dissents. This formula deals only with the first half of all voting combinations, because the second half is the mirror image of the first, as shown above. If a court has three members, then N equals to 1 ($2N + 1 = 3$, therefore $N = 1$). If $N = 1$, then the denominator becomes 4 ($2^{2\times1}$), and the numerator becomes 1 ($[2!/(0!(2-0)!)] = 1$). Therefore, the probability of a judge dissenting in a three-person court is 1 in 4, which is .250. It should be noted that this formula simplifies voting situations and uses many simple assumptions. Therefore, it is not intended to be used to predict the actual votes of judges. Instead, it is designed to calculate the probability of dissenting on courts with different numbers of judges.

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made their private papers available. A perusal of them revealed nothing of relevance for the present study. In a recent book about the Supreme Court's decision-making process, Schwartz (1996, vii–xii) opines that the controversy over the release of Marshall's papers raises serious doubts about whether other Rehnquist Court justices ever will follow suit. Of course, qualitative materials have problems of their own, most notably subjectivity, incompleteness, and "laundering" (Goldman and Lamb 1986, 6–7).

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Aristotle and the Recovery of Political Judgment

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Renewed interest in the concept of political judgment—a mode of reasoning that steers a middle way between abstract, technical reasoning and willful self-assertion—frequently takes the form of a return, qualified in various ways, to Aristotle's concept of phronesis *or prudence. I examine this renaissance of interest in Aristotelian prudence through a study of the works of several leading democratic theorists and Aristotle scholars and conclude that they stop short of some of the very elements of Aristotle's thought that can assist us in promoting prudence. In particular, I try to show that these contemporary advocates of political judgment undermine the case for political* science *(Aristotle's primary concern) and thereby risk losing the independent judgment that Aristotelian political science supports. They fail to give sufficient weight to Aristotle's claim that* phronesis, *while distinguishable from scientific reason or philosophy, functions best even as a practical science when it accepts certain of philosophy's guiding insights.*

One of the central problems confronted by modern political thought, as well as by political life generally, is the relation of theory and practice. The modern attempt to apply theory to political practice, begun by the Enlightenment, culminated in the proclamation—first made by Hegel and, in a somewhat different fashion, by Marx—of the "fusion" of theory and practice. Subsequent severe doubts about the overall systems of these two thinkers, however, have bred a reaction to this claim. Current thinking tends toward the contrary view that there is a "natural and *unbridgeable* gap between theory and practice" (Beiner 1994, 688, emphasis added). And as this view, fanned by postmodern doubts about science or rationality as such, gains stature, it will inevitably threaten the enterprise of political science, at least insofar as it wishes to be a genuine science that serves practice. For if there is *no* link between theory and practice, political science cannot help and may even harm the political life that it studies.

Convinced of the existence of such a gap, yet unwilling to leave political practice to the vagaries of "accident and force" (*Federalist* 1), a diverse and increasingly influential group of political scientists, sociologists, and classicists has emerged in recent years that views pure theory or science as ultimately a bad influence on democratic political life, and so seeks to replace it with a different form of political thinking: political judgment. Unlike earlier religious or existentialist critics of science, those in this group resist irrationalism and oppose any retreat into apolitical self-assertion or self-abnegation. Instead, they seek in political judgment a form of reasoning that emerges from practice and so will reinforce, rather than undermine, politics. In doing so, many of them have felt compelled to return to a single, premodern figure for guidance: Aristotle. They sense in him a rich appreciation of political life and of the thorny question of the relation of reason to politics, often lacking in modern or Enlightenment proponents of reason. Above all, many of them find in his concept of *phronesis* (prudence)[1] a model for truly political judgment, a mode of reasoning that at once avoids the exclusivity of Plato's philosopher-king and the rigidity of modern rationalism. By denying science the right to impose, in however benign a manner, undebatable political "solutions," political judgment encourages citizens to participate in, and thereby to strengthen, political life.

The scholars discussed here—including the communitarian democrat Benjamin R. Barber, the social democrat Ronald Beiner, the liberal democrat Steven Salkever, and the civic republican William Sullivan, among others—certainly do not exhaust the list of contemporary proponents of Aristotelian prudence. But it is appropriate, I believe, to focus on this group, for they attempt to bring about a practicable marriage of sorts between Aristotle and "our" world. Unlike the neonaturalist Roger Masters, for instance, they do not require that we first embrace Aristotelian natural science before recovering *phronesis* (Masters 1989, especially 179–83; see also Arnhart 1981, 1995). And unlike the philosopher Martha Nussbaum, they do not reject Aristotle as being too "elitist" to assist liberal democracy (1994, 101). I have tried to focus on those scholars who argue that Aristotle's political psychology can fairly be applied to the modern world without, as Aristotle no doubt would prefer, revolutionizing it.

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[1] Prudence is not an altogether satisfactory translation of *phronesis*. Prudence, in the modern sense embraced by everyone from Machiavelli (see *Prince*, chapter 15) to President Bush, suggests a pragmatic posture toward politics, unencumbered by any "vision thing" or principle that leads one to tilt at windmills. *Phronesis* encompasses but vastly transcends this view. It is indeed a "practical" (as opposed to contemplative) science; but Aristotle distinguishes it from mere shrewdness by associating it with moral nobility, suggesting that *phronesis* aims not at mere life ("security") but at having us live "well," bringing us closer to our ends as humans. In order to do that, I will argue, *phronesis* will have to rely in some measure on political *science*, in part a discovery of contemplative reason. It is a less reactive and more comprehensive way of thinking than is connoted by "prudence." Unfortunately, I have discovered no more apt English translation and so adopt the conventional one.

Interest in political judgment arises from a widespread sense that something in our political culture and its principles tends to frustrate the exercise of prudence. The diffusion of responsibility among competing branches of government; the growing scope of not-to-be-compromised rights, enforced by unelected and unaccountable judges; the omnipresence of self-interest; the dominance of technical rationalism or expertise, deaf to political nuance or the benefits of a messy compromise: These and other trends have led to a situation in which the exercise of prudent judgment is seen either as willfulness or as naive reliance on "mere common sense" or "anecdotal evidence." And so, according to one of political judgment's leading advocates, "absolutely fundamental questions of human existence" (Beiner 1983, xv) are ignored today. And, on the postmodern assumption that neither religion nor philosophy can any longer address such questions, they must be addressed in "political discourse"(p. xv; cf. Aristotle, *Nicomachean Ethics* 1141a20–2).

To prepare the way, several recent efforts have been made to isolate political judgment, a kind of reasoning that will be more compatible with politics than is theoretical reason (see Barber 1984, 167–73, 198–209; 1988, chapter 8; Beiner 1983; Steinberger 1993; Sullivan 1986, 155–80; Wolin 1989, 100–19). These efforts, though diverse, share a common view that institutions and abstract reason need to be supplemented by a kind of reasoning that counters rather than replicates their moral neutrality. Naturally, they turn to Aristotle in their endeavors, but with this proviso: They wish to free his valuable notion of *phronesis* from the sometimes inconvenient broader theories of both mind and politics to which he connected it. I attempt in this study simply (1) to show how Aristotle can help in uncovering this mode of thinking that falls between scientific or technical rationalism (or historical determinism), on the one hand, and willful self-assertion (or relativism), on the other and (2) to indicate why Aristotle, in contrast with this group of contemporary advocates, refused to sever prudence from philosophy or science. In brief, I hope to show that Aristotle avoids the twin extremes of the Hegelian/Marxist "fusion" of theory and practice and the contemporary attempt wholly to separate them.[2]

LIBERALISM AND THE DEMISE OF POLITICAL JUDGMENT

The central concern of this group of advocates is that our rationalist liberalism, unable to "elude the seductiveness of logical extremes" (Barber 1988, 39), leads to "hyperliberalism" (Beiner 1992, 108), wherein judgment is immobilized by the "rights explosion" or forced by philosophy-inspired judges, bureaucrats, and democratically unaccountable "experts" into Procrustean beds (see, e.g., Glendon 1991, Walzer 1981). Under such conditions, it should come as no surprise that political judgment—judgments made not only *about* politics, but *in* a political spirit, prudently pursuing security and justice in a real community rather than rigorous consistency or intellectual elegance in one's mind—has fallen out of favor.

Aristotelian *phronesis* is essential to politics because it, unlike abstract theory, is flexible, practical, and the product of a shared understanding concerned with particular, concrete human beings. The dispute between Aristotle and his contemporary advocates concerns the extent to which philosophy or *theoria* encourages or undermines such admirable traits. According to the now dominant view, theory is rigid, abstract, and the product of individual, morally disengaged minds concerned with "fixed universals" and not "care" (Beiner 1983, 75–9, 85, 106; also Barber 1988, 151, 205; 1994, 692; Oakeshott 1985, 320–1; Walzer 1981, 393). Scientific reasoning is really "solitary" reasoning and so fosters individualism (Beiner 1983, 16). Whatever its findings, scientific reasoning is at root productive of essentially self-interested citizens. For however well it might lead one to *understand* the common good, this mode of reasoning lacks the sympathy or insider status[3] required to promote or sustain it: Political judgment is "mutual" deliberation (Barber 1988, 200; Beiner 1983, 139). Hence, in this view, political actors cultivate prudence in part by steering clear of theory.[4]

Other proponents of *phronesis*, like William M. Sullivan, claim instead that theory serves a vital, if subordinate, moral purpose: By encouraging us to "transcend a purely egocentric point of view," theory is a necessary preliminary step to prudence (Sullivan 1986, 66; also Barber 1984, 171). Theory supplies a crucial self-transformation that, as long as it is linked to "those concrete loyalties which provide a standing place for reflection and criticism," can generate political prudence (Sullivan 1986, 170–1). Prudent actors can abide philosophy as long as it views membership in a "just community" (and not contemplation) as "the fullest expression of human nature" (pp. 172–3; see also Beiner 1992, 118–9). Thus, theory is here to be promoted, but only as a part of civic education, not as the contemplation of the truth.

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[2] Rather than focus on a narrow argument or text in Aristotle, I will range somewhat freely through his two major political works, the *Nicomachean Ethics* and the *Politics*. For Aristotle, agreeing that "we do not need theories of prudence, we need prudence itself" (Beiner 1992, 179), tries to present not a "theory" of prudence but an account of it, seen from a series of differing practical and theoretical perspectives.

[3] See Beiner 1983, 160. Walzer (1987) makes the helpful distinction between connected and unconnected social criticism. The "unconnected" critic (like Jonah or Plato's philosopher king), taking a transcultural view, is condemned to find all communities unsatisfactory ("caves"). The "connected" critic (like Amos), engaged in the "elaboration and affirmation" of a culture (p. 40), advances constructive criticism only (however radical he might appear to his fellow citizens and however destructive of existing power relationships he might be). Political judgment, it is now widely assumed, must be "connected" criticism.

[4] Aristotle at first also seems to distinguish philosophy and prudence by presenting the apparently stark alternative of "philosophers without prudence" (such as Thales and Anaxagoras) and "statesmen without wisdom" (such as Pericles) (see Tessitore 1996, 50; *Nicomachean Ethics* 1140b8–11, 1141b4–8). The alternative is less stark than it seems, however: Thales was intimately involved, at times, in Greek politics, and Anaxagoras was a personal advisor to none other than Pericles. I will try to show, as Tessitore says, that between these two extremes lies an "unarticulated middle ground occupied by Aristotle himself" (p. 50).

This approach nicely captures the hope of some that political judgment, by focusing on *practice*, will enable us to have a fully just (even if theoretically inconsistent) community. It is precisely because of its focus on practice, however, that Aristotelian prudence leads to the conclusion that "even the best political order will not be simply natural or simply just" (Bartlett 1994a, 385). Aristotle, however, teaches that one can exercise *phronesis* even in a world where politics does not complete human nature.

Finally, Peter J. Steinberger, doubtful of the coherence of Aristotle's notion, advances his own view of *phronesis* that ascribes to it all the strict rationality of *theoria* itself (1993, 126–7; but cf. 279). By yoking the two together under the rubric of "judgment," he tries to ensure both that practical reasoning will be thoughtful and that theory will be relevant and sensible (pp. 282–3).

I will argue that each of these views, helpful though they are in recovering an understanding now difficult of access, fails to appreciate a key element of Aristotle's concept of *phronesis*. The first view misses Aristotle's insistence that *theoria* can foster flexibility, by helping prudence to gain critical distance on popular but misguided views and to resist the often rigid moralism of the community or regime.[5] For, Aristotle argues, one can only know what prudence might accomplish if one also knows what is best simply (a theoretical insight), as well as what is fitting for a particular regime (*Politics* 1289a11–2). Such prudence, while attentive to the traditions and concerns of the community, will also keep in view the broader teachings of political science, chiefly the demands of the "best regime" (the cultivation of genuine virtue), which may involve "unlearning" what one's culture "knows" in order to grasp what "circumstances will allow" (*Politics* 1289a4–5, 1288b27). Against this first view, then, I argue that some transcultural element of *theoria* will be essential to the wise exercise of political judgment.

Sullivan argues that theory counters self-interest or "egocentrism." If left unchecked, however, it has "potentially subversive political implications" (1986, 68): It ascends to a contemplative life that is unconcerned with moral and political life. Aristotle, of course, prefers healthy regimes (where rulers are devoted to the common advantage) to deviant ones (where they are self-interested). But he does not simply oppose self-interest. He stresses, for instance, that economics is a part of *phronesis* (*Nicomachean Ethics* 1141b30–2). And while economics is not quite "egocentrism," it is, at least in part, concerned with securing what is necessary for one's own good life (*Politics* 1256a15). In fact, prudence is concerned "most of all" with matters relating to the person "in whom it exists and *with him only*" (*Nicomachean Ethics* 1141b30–1, emphasis added).

Aristotle's *phronesis* retains some of the "egocentrism" that theory leads us to forget.[6] He is thus less enamored than is Sullivan of the virtues of "responsible *common* life" (1986, 172, emphasis added; consider *Politics* 7.1–3) for it is "absurd to regard politics or prudence as the most serious thing" (*Nicomachean Ethics* 1141b1, 1141a21). Sullivan, concerned that theory can lead us to forget politics (see Plato, *Gorgias* 484c–d), insists that we must uncritically submit, at some point, to "certain understandings that . . . define the self and shape the harmony of its projects" (1986, 106n20). Against this view, I argue that Aristotle requires prudence precisely because no political project is as "harmonious" as it first appears (see *Politics* 1283b27–9; also Beiner 1990, 80; Salkever 1990, 72–3; Yack 1993). Aristotle, unlike Sullivan, does not allow his practical or moral concern for justice to silence the theoretical investigation of what justice is (and cannot be).[7]

Must theory, then, always prove too alluring for those who would otherwise exercise *phronesis*? Most of those who promote a return to Aristotle seem to seek his warrant that, in fact, *phronesis* is *not* the poor cousin of theory. Many argue that *phronesis* is simply an alternate form of intellectual virtue, whose lesser degree of precision is justified and even demanded by its subject matter (Barber 1984, 169–70, 184; 1988, 10). Indeed, Steinberger flatly denies that *phronesis* is in any way less precise or rigorous than theory (1993, 117, 279; but cf. 225). Even after uniting the two concepts under the single rubric of "judgment," however, Steinberger insists that only one side of judgment (judgment as "intelligent performance" instead of contemplation) is applicable to politics. Both Barber and Steinberger imply that political judgment is in no way weakened by ignoring theory proper (cf. Swanson 1992, 216–7). Aristotle repeatedly tries to show, however, that political life devoid of philosophy either becomes an amoral power struggle or mistakenly submits to the "charms of civic republicanism" (Salkever 1990, 148, 141–2), the view that participation alone guarantees wise or at least legitimate policy.

THE NEED FOR POLITICAL JUDGMENT

Despite their stated intention to bracket theory, the advocates of political judgment begin (as, I would argue, they must) with a theoretical claim. They argue that liberalism's concept of asocial man is false (because harmful?) and must be replaced with something like Aristotle's political animal. Such a being, it is argued, would naturally be linked to a mode of reasoning that avoids both the abstractness of scientific reasoning and the arbitrariness of subjectivity. Such reasoning, it is hoped, would supplement or correct our more characteristic economic or bureaucratic thinking while avoiding the dangers associated with individual (or, we might add, majoritarian) capriciousness.

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[5] I employ here and throughout the standard translation of Aristotle's *politeia*. For Aristotle, *regime* is a neutral word employed by political science to describe the constitutive principles that animate both state (or government) and society (or political culture). None of the sometimes offensive modern connotations are implied or are to be inferred.

[6] Aristotle even gently mocks those who are "wise" without being "prudent," i.e., who do not think about what is expedient for themselves. In mentioning here "Anaxagoras and Thales and others like them"—namely the *pre*-Socratic philosophers (*Nicomachean Ethics* 1141b4–5)—Aristotle implies that his *political* (or Socratic) philosophy is prudent philosophy. Because it *does* take self-interest into account, prudent political philosophy usually engages in politics only indirectly (see Tessitore 1996, 133n37).

[7] As Bartlett (1994b) demonstrates, Aristotle concludes that not even the best regime can satisfy, in every particular, the demand for justice.

What political changes does this portend? In its most modest form (Steinberger 1993), the efforts to clarify the concept of political judgment entail no necessary fundamental alteration in any political way of life. According to this view, the concept of political judgment is "essentially neutral" on "specific political arrangements, procedures, or policies" and even on "the purposes of political society" (pp. 300–1). Political judgment accepts political arrangements and their related way of life as given and seeks merely to clarify them. Far from being a form of transcultural theory that might correct or guide a regime from some vantage point outside it, political judgment, in Steinberger's view, implies that "there is no possibility of asking truly critical or radical questions about the framework itself" (p. 297). As a result, political judgment, according to this view, is compatible with any type of politics.

To grant political judgment immunity from the challenges of philosophy in this way may appear to protect sound and tested political practices; however, it also runs the risk of rendering judgment a neutral tool, unable to check practices that are no longer sound. Steinberger alludes to this issue when he argues that "neutral" political judgment need not, for example, render German citizens unable to condemn and resist Nazism. His assumption for making this claim is that, although political judgment ought, nay must, use only the moral and intellectual resources at hand within the given culture, every culture (or at any rate every culture not "profoundly ignorant of right and wrong") contains within it resources adequate to supply any argument opposed to the reigning orthodoxy with an indigenous basis (see pp. 291, 297–8). No culture is as univocal as it appears. This claim seems historically suspect and may be politically naive, for every regime demonizes to some extent what conflicts with its principles (as "foreign gods"). (And, if it demonizes philosophy, it risks remaining in some respects "profoundly ignorant of right and wrong.") Genuine political judgment cannot declare that it simply is "following orders"; it must evaluate that on behalf of which it judges.

Perhaps, then, political judgment supports some particular regime. Salkever (1990), for example, argues that Aristotelian prudence supports liberalism. In fact, he says, there is a "fundamental agreement" between Aristotle and modern liberalism, "at least insofar as each is opposed to arbitrary restraints on inquiry and individual freedom" (p. 8). To the extent that Aristotle defends the rule of law *after* demonstrating its essentially arbitrary character, however, this claim fails to convince: The rule of law, though politically indispensable, is inescapably more arbitrary and even hostile to free inquiry than a freethinker might wish (see *Politics* 2.8, 1253a31–3; see too Salkever 1990, 102). Aristotelian *phronesis* also occasionally accepts arbitrariness in the name not of freedom (as Locke might) but of virtue.[8]

Most advocates of political judgment, however, connect it to participatory or socialist politics. They want to recover and mine the resources of an alternate political tradition, also said to stem from Aristotle: civic republicanism and/or participatory democracy, a tradition that, in their account, was more friendly to political judgment or prudence than is the liberal tradition (see, e.g., Beiner 1983, 152; Pocock 1975; Sullivan 1986, 17–22, 181–4). Here the themes of participation and citizen virtue (the ability to resist the allure of happiness in the name of the common good) are advanced along with political judgment. As the political ramifications of political judgment remain contested, then, let us make a further effort to clarify what political judgment is or offers.

The advocates of political judgment, on both the political left and right, want to isolate it from theoretical (or instrumental or scientific) reasoning and to argue that only the former is relevant to or compatible with politics. But, as I hope to show, these advocates mistake Aristotle's intention in distinguishing *phronesis* from *nous* (insight) with the result that they demand more judgment of citizens than is reasonable and then deny them the philosophic education that alone might provide them with all of which they are capable (see especially Barber 1992).

A key difference between the advocates of political judgment and Aristotle is that they, but not he, insist that all citizens exercise it. Because Aristotle argues both that we are by nature political animals and that prudence is a virtue, the advocates conclude that prudence must necessarily belong to good citizens. Aristotle, however, does not draw that conclusion. In fact, he expressly asserts that "the serious ruler is good and prudent, while the [excellent?] citizen is not necessarily prudent" (*Politics* 1277a14–6; and 1277b25–6).[9] This statement seems to imply that citizens are "good" insofar as they heed the judgments of the ruler, not insofar as they exercise it themselves. Unfortunately, Aristotle does not make clear why this is so. Perhaps prudence requires a level of mental skill beyond what would be reasonable to ask of most people. Or perhaps prudence, insofar as it resembles "cleverness" (*Nicomachean Ethics* 1144a23–9, b1–5), is too morally risky for most citizens to dabble in: The good citizen ought generally to reject the prudent (Machiavellian?) scheme as "wrong." In either case, he implies that a citizen can be excellent (or even, as we later see, philosophical) without exercising political judgment (see Swanson 1992, 142–4).

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[8] Note especially his readiness to include *unnatural* slaves in his best regime (*Politics* 1330a32–3; see Bartlett 1994, 385; Salkever 1990, 143).

[9] I here follow the 1298 Vatican (Vm) manuscript used by Oxford Classical Texts.

In the passage immediately following that quoted above, Aristotle raises the question of how a ruler's education might make him prudent. He begins by noting that "some say"—in a manner reminiscent of the advocates of judgment under review—that the ruler (though not necessarily the citizen) should avoid theory. Quoting Euripides, Aristotle presents as the ruler's motto: "No subtleties for me, but what is needed for the city" (*Politics* 1277a19–20). According to this view, the prudent ruler will eschew liberal education, presumably including even the plays of Euripides, in favor of "riding and war." Thus, prudence, although it may involve a certain loosening of strict citizen morals (see *Nicomachean Ethics* 1144a20–22), seems to involve the suppression of any theoretical concerns.[10] Yet, Aristotle, in the remainder of the *Politics*, continues to point his reader toward philosophy (*Politics* 1279b14, 1282b23, 1324a25–1325b31, 1331a17, 1334a32–4, 1340b5–7, 1342a30–2). Why?

Aristotle finds that philosophy deepens and broadens prudence, whether exercised for political or private ends. Political prudence, concerned with guiding and sustaining the city (see *Nicomachean Ethics* 1141b23–30), is called "architectonic" (*Nicomachean Ethics* 1141b25; *Magna Moralia* 1198b6). While this seems to suggest that prudence should guide or limit philosophy for political purposes, Aristotle expressly denies it (*Nicomachean Ethics* 1145a6–12). Architecture, after all, is guided by our prior needs (e.g., for houses). Thus, political prudence needs philosophy at least to this extent: Only those who comprehend human nature can judge how to arrange and satisfy our ends (thus Aristotle calls the political philosopher the "architect of ends"; *Nicomachean Ethics* 1152b2–3).

If the ruler is to have political prudence, and the citizen none, then who precisely is to possess the other type of prudence, which involves the "kind of wisdom which is concerned with oneself" (*Nicomachean Ethics* 1141b30–1)? The answer seems to be the very type of person who is portrayed rather unfavorably throughout the *Politics*: the private individual. Far from suggesting that prudence thrives only with political participation, Aristotle here claims that "the man who knows and minds his own business is prudent" while politicians are "busybodies" (*Nicomachean Ethics* 1142a1–2). A prudent person might then wish to withdraw from political life in order to philosophize, while accepting, as a sober second-best choice, the least degree of involvement compatible with his or her own good (see *Nicomachean Ethics* 1142a9–10). The first sense of prudence is political—as Beiner, Sullivan, and Barber require—but it is not for the average citizen, while the second sense, more the aim of a private (if not average) individual, is apolitical. If there is a case to be made for a prudent, participating citizenry, Aristotle does not make it.[11]

Salkever (1990) attempts to resolve this problem by combining a communitarian elision of the public and the private spheres with a liberal acceptance of any "intelligent balance" of our competing needs and interests (pp. 141, 138). In his interpretation, such flexibility assumes that Aristotle equates justice with individual "long-term interests" (pp. 75, 141; see too Bickford 1996, 400n3). This reading is based on Aristotle's claim that human speech concerns "the expedient and the inexpedient, and *therefore* also the just and the unjust" (*Politics* 1253a14–15). Not only does this interpretation contradict the overall sense of the *Ethics*—which is that the just is concerned with the noble not the expedient—it leaves us unable to distinguish prudence from cleverness. By blurring the distinction between the two types of prudence, Salkever is led to claim that *phronesis* is "rationality about our interests" (which, he argues, can be determined politically or privately; 83n54; cf. 88). By dismissing too quickly the noble instinct to act against our interests, he obscures the fundamental Aristotelian question of whether political life is itself in one's "long-term interests" (in the sense of fulfilling the deepest human needs). The choice of the best way of life is, for Aristotle, a soluble theoretical problem, not only for "prudent" people to determine in ways that will "vary widely" (Salkever 1990, 19; see also Arnhart 1995, 393 on prudence as determining "some" conception of a whole life).

While Aristotelian prudence need not entail political participation, it cannot for that reason become, as Hobbes wanted (see next section), a strictly apolitical or private affair. It must still consider political questions but from a broader perspective. That perspective would not sacrifice close analysis of the specific situation on the altar of abstract principle. It would however, see that, because the character of expediency itself changes according to the regime (see *Rhetoric* 1360a17–37), the thoughtful person will discover that the standard of "what works" is inadequate. Moreover, *phronesis* must determine whether and to what extent involvement in political life is called for. Of course, those who deliberate well concerning themselves will learn that they must also deliberate well concerning what is good for others (cf. *Nicomachean Ethics* 1140a26 and b10) since "perhaps one's own good cannot exist without economics nor without some regime" (1142a9–10). But this remark stops well short of claiming that human nature is *realized* through engagement in (republican) political life. It suggests only that a prudent person will reflect on (and hesitate to weaken) that level of political security and decency that will enable him or her (and others) to secure the good, even if that good is philosophic self-sufficiency (see Plato, *Laws* 875a–b).

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[10] This "closed" aspect of politics is understood differently by Beiner (1983, 139) and Sullivan (1986, 173) who view Aristotle as arguing for the "self-sufficiency" or comprehensiveness of politics. But Aristotle says that politics can attain self-sufficiency only "so to speak" (*Politics* 1252b29). If the city is at all lacking in proper "judgment concerning the advantageous things and the just things" (for which, as we shall see, philosophy is required) then it is "impossible" for it to be "simply self-sufficient" (1328b14–7). Accepting any polity's claim of "self-sufficiency" is dangerous insofar as it could mask the "profound ignorance of right and wrong" feared by Steinberger.

[11] Nichols makes a thoughtful argument for Aristotle's reliance on the "collective virtue and prudence" of the many (1992, 82, 147). Yet, even she is forced to concede that the statesman must "guide" or "direct" the contributions of the many toward common goals (pp. 66, 82) and that prudence is "the statesman's distinctive virtue" (p. 83). Consider also Aristotle's praise of the *least* participatory sort of democracy (*Politics* 1318b5–15, 1328b24–1329a2).

THE LIBERATION OF POLITICAL JUDGMENT?

The key post-Aristotelian change in our understanding of political judgment occurs in the thought of Thomas Hobbes. Hobbes's hostility to Aristotle is well known; but the fact that a central aim of his critique involved the effort to "liberate" political judgment (or *phronesis*) from the tutelage of philosophy has gone largely unexamined. According to Aristotle, *phronesis* is in part an intellectual virtue (*Nicomachean Ethics* 1103a5–7), one that comes into its own (bringing the moral virtues in turn to fulfillment) through the mind's ability to grasp correctly the good for human beings: "*phronesis* is necessarily a truth-attaining rational quality" (1140b20–1, 1113a29–30, 34; cf. Barber 1988, 198). Prudence takes some of its bearings by *nous* (insight) or philosophy (*Nicomachean Ethics* 1143b1–10) and, as such, is a rare quality. For Hobbes, in contrast, the absence of any greatest good for humans renders deliberation merely the final link in a train of thought leading to whatever action is taken ([1651] 1968, 127). More important, Hobbes argues that, because egalitarianism, if not the facts, requires it, all humans, not just the rare man of "genius," could be prudent (p. 211; Berlin 1996, 26). Thus, for Hobbes, prudence becomes "but Experience; which equall time, equally bestowes on all men, in those things they equally apply themselves unto" ([1651] 1968, 183). Prudence is nothing more than gaining a "knack"—learning the ropes—in this or that discrete situation. And so, emphatically departing from Aristotle, Hobbes concludes that "Prudence . . . is not attained by Reasoning," which produces only "general, eternall, and immutable Truth" (p. 682).

In rejecting Aristotle, then, Hobbes not only rejects teleology but also, and crucial for our purposes here, seeks to liberate prudence from the claims of philosophy. Prudence becomes practical reasoning with a vengeance, refusing any guidance from philosophy.[12] And a practical, not theoretical, concern is the primary motive for this change in theoretical orientation. For Hobbes not only needs prudent human beings in order to forge a social contract, he must also, by uncoupling prudence from philosophy, help to ensure that deliberations concerning the good never ascend to the level of what is good for human beings simply, lest politics return to the disputatiousness from which he sought to free it.

Thus, the anti-Aristotelian Hobbes appears to accomplish, with respect to prudence, much of what its contemporary advocates desire. Anticipating their key demand, Hobbes consigns prudence and philosophy to two disparate realms. Indeed, the only "truth" that philosophy is to teach in a well-ordered commonwealth is that there are no philosophic teachings that can inform or restrict the sovereign's judgment ("no Law [made by the Sovereign power] can be Unjust" as there is "no Court of naturall Justice," [1651] 1968, 388, 394). By insisting on this feature of political judgment, Aristotle's advocates leave one foot in Hobbes's camp. In fact, while Hobbes is at least as guilty as is Aristotle of failing to universalize political judgment, only the latter is criticized for it (Beiner 1983, 103): Since Hobbes allows for prudence without philosophy, it is in the spirit (if against the letter) of Hobbes to democratize political judgment.

By freeing political judgment from rationalism's self-confidence, the advocates of judgment might seem to be trying to bring the benefits of Socratic doubt to all. At least, they hope to see less "vehemently serious" citizens (Salkever 1990, 104, ascribes this hope to Aristotle rather than Hobbes). But, again, the political judgment promoted turns out to be more rigid or decisionist than advertised. For "the individual is a paltry vessel when torn apart from within by doubt and vacillation" (Barber 1989, 66). Enlightenment absolutism, it seems, is rejected only because it can be had in private, not because it is absolute. The advocates have no objection in principle to post-Enlightenment absolutism, that is, to the imposition of a truth conceded—or created—by all. That some individuals are *not* "torn apart . . . by doubt and vacillation," even when judging the thorniest moral and political issues (see Barber's own excellent appreciation of Harry Truman in Barber 1988, chapter 5), however, seriously weakens the argument that judgment, to be genuinely political, must be held in common. For some people, apparently, do not need participation as a source of "mutual *consolation*" (p. 67, emphasis added).

Because most of his contemporary advocates do not accept Aristotle's claim that contemplation is the end of human life (see *Politics* 1253a27–8, 1323a14–5b31; cf. Beiner 1983, 118–9; Sullivan 1986, 165; and, for a helpful exposition of Salkever's somewhat ambiguous position, Bartlett 1994a, 394 n13), they feel compelled to make political life (and its concomitant practical virtues) more fulfilling than Aristotle does. Where Aristotle says that *phronesis* ultimately ministers to contemplation (see *Nicomachean Ethics* 1145a7–12, 1177a12–79a33; *Magna Moralia* 1198b12–20), the advocates want it to be an end in itself. For Aristotle, virtue is the end; prudence (the observed ability to secure the good) merely grants "authority" to virtue (*Nicomachean Ethics* 1144b17–8 and b21–22). Thus, even those who lack *phronesis* (in the political sense) can still attain virtue through their own efforts if others in authority exercise it (consider *Politics* 1325b10–12).

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[12] Contrary to first impressions, this makes Hobbesian political judgment more, not less, dependent on abstract scientific "laws" of "social mechanics" (Berlin 1996, 26). As his contemporary advocates note, Aristotle sought to protect political judgment from such misguided and abstract rigidity. Contrary to their further claims, however, Aristotle relied on philosophy to keep political judgment from making this wrong turn.

Hobbes's modern heirs, however, are no longer willing to pay the price Hobbes demanded in offering us his instrumental prudence of means (veritably unlimited in scope)—namely the abandonment of the debate about ends or goals. They, along with Aristotle, want reason to help us to consider our ends as well. For it has become increasingly apparent that proscribing any discussion of ends will have the effect (sought by Hobbes) of leaving the pursuit of "commodious living" as the end by default, spelling the end of "meaningful" politics.

PRUDENCE, MEANS, AND ENDS

Does Aristotelian *phronesis* deal with ends as well as means? An enduring interpretive tradition claims that it does not but, rather, deals only with means to set ends. *Phronesis* is now being asked by some of Aristotle's contemporary defenders, however, to reason about the most fundamental human questions or even to "construct" ends themselves (see, especially, Beiner 1983, 95, 152; Steinberger 1993, 241). The very questions that, when debated by philosophers and other partisans, had led Hobbes to sever prudent politics from philosophy, are now being reintroduced into politics (as opposed to private discussion) by many contemporary proponents of political judgment. Aware that Hobbes would condemn this development, the proponents assume that they must then be fulfilling the intention of his chief enemy, Aristotle. But are they?

At least one advocate realizes that this may ask too much of political judgment. Beiner concedes that only "philosophical, or perhaps only religious, experience" can be comprehensive (1983, 165). If this is so—and I suspect that it is—then political judgment cannot provide what pluralistic liberals are said to lack. Unable to take a comprehensive view of the whole, political judgment would supply quick fixes at best. This is presumably one reason why Aristotle states that it is the political philosopher and not the prudent statesman who is the "architect of the end of man" (*Nicomachean Ethics* 1152b1–3).

If *phronesis* cannot generate goals, perhaps it may itself "constitute a goal" (Salkever 1990, 76n44); that is, there are many competing goals in human life, none of which is superior to the others (pp. 87, 138, 156). To balance them all prudently, as best as each of us can in our given circumstances, is the only "goal" of which we can speak cogently (1991, 34–5). Susan Bickford has elaborated this notion into the claim that Aristotle viewed political deliberation as a good for humans as such—as an end in itself (1996, especially 415–8).

Bickford's key example, however, tends to weaken her argument—and does so in a way that helps us to see why Aristotle asserts that *phronesis* needs philosophy. She cites as a good example of *phronesis* the efforts of Nicias to dissuade the Athenians from undertaking the Sicilian expedition. Nicias is a good example of someone who never deliberated about ends: Thucydides tells us that he regulated his whole life by "law-bred virtue" (*Peloponnesian War* 7.86.5). But Nicias fails to quash the expedition to which he objects and then goes on to scuttle its chances of success. Neither Nicias' decency nor his respect for the giving of advice (Bickford 1996, 415) can make up for his lack of understanding, either of his fellow citizens or of strategy. He is, in Aristotle's language, merely continent, while his rival Alcibiades, the erstwhile student of philosophy, is shrewd but incontinent. Thus, neither illustrates *phronesis* as Aristotle understands it. And this helps us to see what Salkever and Bickford miss (and what Beiner senses but resists): There is no escaping the need for a comprehensive account of human life (especially of *genuine* virtue) and only philosophy can provide it.

Aristotle does indeed want *phronesis* to reason about ends or goals, but only in a political or limited manner. For the ends contemplated by theory (the best regime simply) are too broad to solve the problems of a particular place and time (which is in need of the best possible—or perhaps only the "easiest and most attainable"—regime; *Politics* 4.1). Theory can, to cite the most relevant example, determine that "natural right exists," but since "all of what is just is subject to change" (*Nicomachean Ethics* 1134b29–30), it remains to be determined in every particular case what end (e.g., mere life or living well) should take precedence. But if theory cannot generate precise ends, and if we are further forbidden to exercise *phronesis* about ends, then we are in effect condemned to accept the ends set by the gods (or their spokesmen) or the majority or history. Aristotle thus encourages *phronesis* to reason about such things as the relative importance of (potentially conflicting) ends, and even about the full implications of an end perhaps only dimly perceived at the start.

If, however, *phronesis* is allowed to construct ends or goals, free of theory's guidance, two crucial elements of Aristotle's thought are lost: (1) The most important human ends (virtue and happiness)[13] are natural, and (2) politics generally works best when its ends (and the law) are unchangeable. Aristotle suggests that our inability to withstand pleasure or pain makes us lose sight of first principles (*Nicomachean Ethics* 1140b17–8), tempting us to forsake a given (difficult) goal and, instead, to substitute "prudently" an easier one. But a prudent statesman should never deliberate about "whether he should enact good laws and enforce them" (*Nicomachean Ethics* 1112b14–5). He may only consider how to pursue noble goals in the "easiest and noblest" manner (1112b17). It is thus Aristotelian political judgment of the highest order when Abraham Lincoln, though sympathetic to John Brown's cause, did not hesitate to condemn his acts as antinomian

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[13] Salkever exemplifies this departure from Aristotle when he states that, for Aristotle, justice is "neither desirable nor natural in itself but only as a way in which 'living well' or simply 'our interest' [*to sympheron*] can be brought into being" (1991, 27). This "Kantian" desire to *work out* morality through the free exercise of autonomous human reason—the driving motive of those who seek a recovery of political judgment (see Beiner 1983, 43–4, 57, 63–70, 72–9, 102–4; Beiner 1989, 231)—represents a radical break with Aristotle.

(and hence self-defeating in the long run, since only respect for law could protect the slaves that Brown and Lincoln both wanted freed).

How, then, does *phronesis* grasp political ends? Unlike theory, it will learn from experience; it will even recognize that reason is sometimes inapplicable to politics (*Nicomachean Ethics* 1142a12–21, 1141b16–8, 1180a9–b12 and *Politics* 2.8). Nevertheless, Aristotle states that it is the political philosopher who is the "architect of the end" for man (*Nicomachean Ethics* 1152b1–2; see also *Politics* 1325b21–4). But this image suggests that the political philosopher will neither construct ends freely nor ignore practical considerations. For, just as an architect could construct any one of a wide but limited variety of homes to meet our (relatively permanent) housing needs, so the political philosopher can design any of a variety of regimes to meet our political and moral ones. Thus, the person of *phronesis* would combine flexibility in respecting different experiences, histories, and locations with a clear grasp of the ends of human nature.

Toward the beginning of his sustained analysis of the various types of wisdom (*Nicomachean Ethics* book 6), Aristotle does appear to draw a firm boundary between scientific knowledge and prudence (as well as art, wisdom, and intellection) (*Nicomachean Ethics* 1139b15–8). If the objects of understanding can be distinguished into permanent things that exist "by necessity" and transient things that can be otherwise (especially if human effort is redirected), then it makes sense to distinguish scientific knowledge (of the former) and prudence (which deals with the latter). Theoretical wisdom, then, is of "first things" (basic, permanent principles) while prudence deals with "particulars," which can and do vary.

Left thus, however, *phronesis* would be little more than the instrumental rationality against which the advocates of political judgment protest. That narrow (though influential) conception of *phronesis* stems from a misreading of a key phrase in the *Ethics* which states that we do not deliberate about ends, but about *pros to telos* (1113b3–4; see also 1145a4–5, 1112b12, 1113a13–4). While this is usually translated (e.g., by Apostle, Rackham, and Ross) as the "means to an end," thereby lending support to the traditional interpretation, Wiggins (1980, 223–7) convincingly argues that the Greek should in fact be rendered as "what is toward the end." As Wiggins demonstrates, Aristotle encourages deliberation not only about the means to an end, but also about "something whose existence counts in itself as the partial or total realization of the end" (p. 224). Since, in practical life, there are always multiple ends to pursue (e.g., security and freedom, inclusiveness and excellence, etc.), *phronesis* should determine, at any given time, which end to pursue (in light of the resources, not least the moral resources, required to pursue it). This is why Beiner's avowal that there is "no limit" to the possibility of "overcoming incommensurability" (1983, 186n17)—his denial that there are fundamental conflicts—is at odds with Aristotle's political thought (cf. Sullivan's more modest hopes; 1986, 176). The beginning of prudence is the recognition that conflict (of principle as well as interest) is a permanent part of political life (see Salkever 1990, 5).

Most of Aristotle's present-day advocates are alert to the need to keep prudence from becoming a neutral tool. They argue or assume, however, that some form of democratic agreement, not theory, should do the job of guiding it toward justice. But Aristotle asserts that *phronesis* is somehow a "counterpart[14] of intellect": For *nous*, a certain "insight" (akin to sense perception) provides *phronesis*, as well as science, with the particulars with which it must deal (*Nicomachean Ethics* 1142a26–31, 1143b1–11; see Newell 1991, 199). *Phronesis* is thus "a virtue of reason [*dianoia*] which enables men to choose well in regard to good and bad things" (*Rhetoric* 1366b20–22) and, as such, will be the province more of statesmen than of "good" citizens. In fact, Aristotle virtually concludes the *Ethics* by claiming that the prudent ruler must know statesmanship in a more or less scientific fashion, so as to "make [*poiein*]"[15] people better. Thus, it is "the man who knows" who succeeds best in any science "which uses diligence *or prudence*" (*Nicomachean Ethics* 1180b17–28, emphasis added). This does not mean, of course, that Aristotle "brings his argument around" (to use a charge he makes against Plato's *Laws* at *Politics* 1265a2–3) to a defense of the philosopher king. Political judgment, taking on its specific color from the regime in which it operates, would, in the case of liberal democracy, never impose where it cannot persuade, and would strive to educate rather than ignore public opinion.[16]

PRUDENCE AND PHILOSOPHY

Prudence, then, does partake of theory or philosophy.[17] After demonstrating that there are a variety of types of regimes, Aristotle notes that "certain questions are involved, and it belongs to one *philosophizing* . . . and not merely looking toward action not to overlook or omit anything, but to make clear the truth concerning each thing" (*Politics* 1279b11–5; emphasis added). But what can philosophy ("the truth concerning each thing") bring to political life—especially insofar as it is "not merely looking toward action"? Philosophy, according to Aristotle, reveals the incomplete nature or justice of every regime. *Phronesis* severed from philosophy always tends toward partisanship. For philosophy sees what the partisan cannot, namely, that various regimes are usually found "simultaneously" (*Politics* 1283b9) in a city, not because of infiltration by hostile elements but because of the different aspects of human virtue. This philosophic insight might help the prudent actor to resist wanting to "purify" a regime of contrary principles (e.g., Barber's "strong" democracy) but to juggle or accommodate them.

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[14] I follow Wiggins (1980, 235) and, implicitly, Newell (1991, 199) in translating *antikeitai* as "counterpart" rather than "opposite."

[15] Newell (1991, 199–200) points out that, by having the statesman "make" people better ("if it is through laws that we can become good"), Aristotle relaxes the distinction between prudence and "art" as well. The different types of intellectual virtue, that is, turn out, on examination, to be far less easily distinguished than Aristotle at first suggests.

[16] Nor, insofar as political judgment is distributed along a continuum, would Aristotle in principle oppose extending to citizens various limited opportunities to exercise it. He would, I suspect, join with *Federalist* 63 in excluding the people in their "collective capacity" from the exercise of judgment.

[17] This is why "the deliberative rhetorician . . . needs a knowledge not only of the actual subjects of deliberation, but also of *all* the elements of human nature that influence deliberation" (Arnhart 1981, 55, emphasis added).

Political judgment's advocates depend on theory for political help more than they claim, for only a "rationalized" humanity could participate in their version of it. As Beiner himself concedes, the core project of these advocates of political judgment—namely the full "affirmation of deliberation and rhetoric"—requires one to "portray politics as *fully* discursive, which in turn assumes that *all* men are amendable to reason (a post-Enlightenment assumption)" (Beiner 1983, 178 n25, emphasis added). He concedes—contrary to the more prominent claims made for political judgment—that deliberative practice cannot *generate* reason but must presuppose it. Deliberation undertaken by extremist fanatics will hardly issue in *political* judgment. And merely making people more "political" may render them more eager to impose (their view of) justice rather than to accommodate others (see Yack 1993, 222–3, 231–2). Even where men *are* generally reasonable, an increase in participatory deliberation may itself make them less so.[18] As though elevating abstract principle over experience, these advocates simply deny Aristotle's claim that most people are *not* amenable to reason (*Nicomachean Ethics* 1179b5–17).

If political judgment can have no recourse to theory-generated "antecedent moral knowledge"—and so "comes to life" only in practice (Beiner 1994, 690)—then perhaps every citizen is competent to render prudent judgment.[19] At its most extreme, this view demands participatory judging (e.g., nationwide town meetings) on all important issues (see especially Barber 1984). The more reasonable or at least practicable suggestions, however, involve appeals to what Sullivan calls "public philosophy." A public philosophy (or "public reason"; Rawls 1993, 212–54) must involve "publicly adducible reasons or grounds" (Beiner 1983, 8); it must present "public purposes publicly arrived at" (Barber 1986, 61). It is an open-ended, public conversation to which all citizens could, at least potentially, contribute—but only if they defer to "shared" knowledge. The independent thinker who tries to assist us by questioning our shared but misguided beliefs will have no place here. Public philosophy is neither the application of philosophy to public matters nor the "political, or popular, treatment of philosophy, or the political introduction to philosophy" (Strauss 1959, 93). Rather, it is the "coherent public political discourse" arising from republican or self-governing polities (Sullivan 1986, 9). By "interpreting and delineating common understandings" (p. 9; see also Beiner 1989, 235), public philosophy gives statesmen a ready-made rhetoric by which to move citizens.

This undeniable advantage, however, may be outweighed by two disadvantages. First, not every sound or necessary policy decision can be given "*explicit* and *rational* justification," not even retrospectively and not even in this limited sense of "rational" (Steinberger 1993, 239, 222–40, emphasis added; see also Rawls 1993, 213). For human beings, especially in political life, are passionate beings at least as much as they are rational ones (see Bickford 1996, 412). Second, if public philosophy becomes a "restraint" on the statesman's thought (Nichols 1987, 669), rather than an arrow in his quiver, it threatens to restrict political judgment more fully than theory might.[20] Beiner is surely right when he states that, for Aristotle, "one affirms one's membership in a given community . . . by adjusting one's *speech* to the opinions and sensitivities of one's fellows" (Beiner 1983, 101, emphasis added; see Aristotle, *Rhetoric* 1.9.30, 2.2.13).[21] He misses Aristotle's reservations about community, however, when he goes on to assert that "our ends are not merely pursued rhetorically, they are themselves *constituted* rhetorically" (p. 95, emphasis added). The nature of one's audience will, of course, limit the possibilities; but, by refusing to adjust his political *thinking* to his audience's way of *speaking*, "the statesman is less limited by citizens than they are limited by him" (Bickford 1996, 414).

Our agreement with public speech unfortunately does not always guarantee its goodness (or even its ultimate consistency with our principles). We sometimes agree with public speech because it persuades us that it is true; but we sometimes long only to come to some agreement, and other times we are drawn to certain speeches for even darker motives. It is characteristic of the advocates of political judgment to pass over in silence the problem of demagoguery, as though talk of the common good could never serve as a cloak for wicked or foolish purposes. Barber even claims that only politically denuded *liberals* are vulnerable to the demagogue's wiles (Barber 1984, 180, xii). Demagoguery, of course, predates liberalism—and the very success of past demagogues suggests that political judgment is not really that broadly distributed. By trying to enlist philosophy, in the guise of public philosophy, as a community's paid in-house counsel, so to speak, advocates of political judgment deny us our best source of friendly criticism.

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[18] "Had every Athenian citizen been a Socrates, every Athenian assembly would still have been a mob" (*Federalist* 55).

[19] Only Barber *argues* for citizen competence at judging. He claims that with experience, citizens can, like congressmen and presidents, gain a working competence in, say, issues of genetics or nuclear weapons (Barber 1988, 202). His choice of largely technical issues is revealing, however, for prudence comes into its own primarily in nontechnical ones. "Any" adequately trained intelligent citizen can determine whether a given action or policy follows precedent or achieves maximum efficiency; only a prudent judge can foresee and take into account a decision's political or moral effects, thereby advancing a wise policy.

[20] The restrictive nature of public philosophy is clearest in the leadership theories of Woodrow Wilson. His call for leadership should not obscure the fact that, for him, the leader was only to draw out the implicit public philosophy held by the people, not to "form" their opinions as the Founders had allowed. Wilson's "education" of public opinion "did not begin in the minds of the elite but in the hearts of the masses" (Tulis 1988, 102). It should be stressed that Wilson's approach encourages a much more "elevated" (and potentially explosive) political debate than does the Aristotelian political judgment that I am defending.

[21] Aristotle's readiness to make even a qualified accommodation to community is the reason why we need him in the first place. Modern concepts of judgment, deriving largely from Kant, treat accommodation with political life as detrimental to the autonomy of judgment: For Kant, "the autonomy of judgment requires that it *exclude* accommodation to the judgment of the public (in the sense of a particular community), or of friends, both of these amounting to heteronomy" (Beiner 1983, 57, emphasis added).

The core difference between these advocates and Aristotle is that they refuse to accept, or make adequate use of, his division of political justice into "conventional" and "natural" parts. For Aristotle, conventional right concerns those things which are indifferent in themselves (whether to sacrifice "one goat or two sheep"; *Nicomachean Ethics* 1134b22–3) but become standards of just and unjust behavior once a community settles its opinion. This, I suggest exhausts what the contemporary advocates discussed mean by political judgment: a determination made by a community that, having relied on no principle outside itself ("antecedent moral knowledge"), cannot be gainsaid. But Aristotle also recognizes the existence of natural right. By treating the philosopher as a mere dreamer or even a traitor (Barber 1988, 162; Beiner 1983, 115; Oakeshott 1975, 30), political judgment deprives itself of natural right as a different kind of judgment.[22] That kind, of course, involves judgments about things that are far from indifferent to us (e.g., criminal law). Unless, following Hobbes, we agree to settle such things by mere convention, they will be contestable, sometimes deeply so. Aristotle does not claim that there is a "determinate standard or state of affairs, in the natural order or anywhere else" by which we can dispose of the resulting conflict. He does not demand, as Yack points out, that the prudent statesman " 'raise' conventional standards toward natural standards" (1993, 144, 149). In fact, as my earlier reference to Lincoln implied, wise political judgment will try to reinforce the rule of law, not change or improve it. Resisting the urge to impose a wise decision from above (as, e.g., Fichte [1814] advocates), as well as the urge (shared by these advocates) to shore up any communal agreement by refusing to apply any "antecedent moral knowledge" to it, the actor of Aristotelian prudence will try to remind fellow citizens of what they can know to be just by nature if only they can overcome the influence of pleasure, pain, narrow self-interest, unrestrained imagination, excessive hope or fear, etc. This rhetorical approach (excellently outlined in Arnhart 1981), is rejected by all the advocates discussed (except Salkever 1990, 92) as unfairly privileging some morally suspect thinker within the community. Insofar as philosophy undermines confidence in "our" ways, political judgment, nay politics itself, wanes as philosophy waxes (see Barber 1988).

When we examine what it means to assign wholly different tasks to theory and practice, we begin to see the mistaken nature of this undertaking. Beiner offers what he takes to be a telling contrast: Thucydides, the exemplar of the wise but detached political "spectator," and Pericles who, completely identifying with the city of Athens, is the "prototype" of the man of sound "political judgment" (Beiner 1983, 108). Pericles, however, owed much of his exceptional ability to his studies with the philosopher Anaxagoras (see Plutarch n.d., 185–6). In particular, such study rendered Pericles superior to "all arts of popularity" (see Thucydides 2.65.9 where Pericles is described as a monarch in all but name). If Pericles is truly the "prototype" of the man of political judgment, then philosophy's resistance to popular judgments is *essential* to political judgment.

Worse yet for Beiner's thesis that political judgment is "possible" only on the basis of "*common* concerns" (1983, 141, emphasis in original) is the fact that the highest praise for political judgment offered by Thucydides in his work is directed, not toward Pericles, but toward Themistocles (see Thucydides 1.138). Themistocles' extraordinary "natural" talent for political judgment became evident precisely when he was banished from "his" Athens and immediately set himself up as the chief advisor to the King of Persia. His "shrewd judg[ment]" was so much his own private accomplishment—and so little the product of a shared civic consciousness—that it was "transferable" to any regime in which he happened to land (see Strauss 1959, 81–2).

CONCLUSION

The renaissance of interest in Aristotle's concept of *phronesis* proves, in the final analysis, to be motivated less by a desire to recover Aristotle than by the felt need to accommodate both postmodernism's critique of reason and democracy's dislike of political distinctions. Conceived by the thinkers discussed as an antidote to reason (viewed as both harmful and illegitimate for politics), *phronesis* so understood will (by design) undermine both the science of politics and representative democracy. But if Aristotle's *Politics* stands for anything, it is not republicanism, but the science of politics. Aristotle's whole undertaking is premised on the view that the gap between theory and practice is *not* unbridgeable but, in fact, can be bridged by his political science. When it misunderstands political life, theory can indeed bring it harm (see *Politics* 2.8). But Aristotle's response is, first, to encourage theory to understand both itself and political life *better* so as to assist it, at least under certain conditions, and, second, to discourage political life from thinking that it will be on sounder footing by setting aside theory and its grasp, however tentative, of the truth. *Politics*, books 4–6, contains myriad ways in which theory can help prudent democrats, especially by teaching them some crucial political lessons before that harsh teacher, experience, does.

Aristotle differs from the contemporary advocates of political judgment (at least on the question at hand) in this: For them, political judgment is best when it rests on the intuitions of the particular community in which it happens to exist (there are "no standards outside of the regime" for the purposes of judgment [Barber 1988, 17]). For Aristotle, in contrast, political judgment is at its best when it "leans against" the principle of the regime, in an effort to resist the inevitable universalizing or tyrannizing tendencies and to make up for the inevitable blind spots that, unchecked, destroy the regime.[23] One might say that the chief virtue of the "mixed" regime promoted by Aristotle is that it institutionalizes a variety of conflicting first principles, so that several are available to prudent statesmen and even blinkered patriots are constrained to contend with them.

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[22] I rely here on Yack's (1993, 143) helpful suggestion that we treat natural and conventional right as two kinds of judging rather than as two different standards of justice.

The advocates discussed argue that judgment severed from philosophy is judgment respectful of difference; only some form of communal political judgment will transform souls from individuated pursuers of self-interest into citizens who can deliberate with and even defer to others, not taking their stand with the truth as they see it, because they have learned to view the truth itself—in its political aspect at any rate—as a *product* of that common deliberation. Aristotle, in contrast, argues that deference to common deliberation in the absence of standards outside of the community (i.e., the best regime) is but a disguised form of absolutism, made all the more virulent and unforgiving by its apparent sacrifice of self-interest to noble common purposes.[24] Political judgment, in addition to suggesting our best option, must remain open to the possibility that even that option is questionable.

In the end, the advocates discussed fail their own test by threatening the sound practice of politics. For if there is any truth to the common opinion that politics involves the occasional departure from strict morality, these advocates, by asking political judgment of *all* citizens, invite them both to learn how to "bend" morality for political purposes and to moralize with (i.e., restrict) genuine political judgment. Only the comprehensive insights of philosophy can help us to avoid narrow moralizing without descending into amoral or short-sighted shrewdness. Theorists who stress the challenging theoretical aspects of political judgment are more likely to save or strengthen it than are the antitheorists who diminish it by passing it out freely.

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[23] See *Politics* 1310a19–35. Consider also Tocqueville's advice to democratic statesmen to "correct" rather than to "favor" a certain "fatal tendency" of democratic citizens (Tocqueville 1969, 639).

[24] Barber concedes, in a somewhat chilling passage, that the fact that the norms of "political judgment are provisional" does not mean that they are "relative or feeble"—they may, in fact, be "vigorous" and inspire "decisive" action (Barber 1984, 170).

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Positive Theory, Normative Theory, and Practical Politics: Sidney Verba, Kay Lehman Schlozman, and Henry E. Brady's *Voice and Equality: Civic Voluntarism in American Politics*

Voice and Equality: Civic Voluntarism in American Politics. By Sidney Verba, Kay Lehman Schlozman, and Henry E. Brady. Cambridge, MA: Harvard University Press, 1995. 640p. $39.95 cloth, $17.95 paper.

Sidney Verba, Kay Lehman Schlozman, and Henry E. Brady (VSB) have concentrated scholarly attention on the inequality of political voice in America. Employing an innovative survey design that produced a novel data set of political activists, VSB demonstrate that activists and nonactivists have different sets of policy concerns and that different modes of participation send different messages to public officials. In short, the participatory process—which they argue is based on citizen resources, psychological engagement, and access to networks—is biased. More participatory equality would yield a different mix of policy messages to political leaders.

We asked three senior scholars, who approach the problem of participation in America from different angles, to evaluate VSB's work. They declare *Voice and Equality* a modern masterpiece of political science and probe the best they can for weakness. John H. Aldrich emphasizes positive theory. He stresses the limits to assuming that all acts of political participation are part of the same theoretical universe and therefore to constructing a measure of the overall volume of participation. Aldrich argues that only a domain-specific approach to participation (e.g., elections, community action, interest-group politics) that focuses on an actor's goals and his or her interactions with other actors and institutions in the domain can explain why someone is an activist. Jane Mansbridge highlights normative theory. She observes that adversarial theories of democracy suggest that the inequality of participation matters because it affects the "Who gets what?" question, while participatory theories of democracy suggest that inequality matters because it affects the "Who are we as citizens?" question. Mansbridge argues that research must be concerned with both the distributive and educative effects of participation. Finally, Jennifer L. Hochschild stresses practical politics. For example, she notes the limits of VSB's finding that group identity is irrelevant to political activism. Hochschild proposes an empirical investigation of how group-specific factors (e.g., black consciousness) affect participation.

In sum, the reviewers maintain that VSB's general theory of political participation in America must address contextually specific and historically disaggregated factors: particular goals and domains of political activity, the redefinition of political values that occurs during the political process, and group-level forces of identity and consciousness. In a rejoinder, VSB defend their work and yet graciously accept the many useful suggestions made by Aldrich, Mansbridge, and Hochschild. The discipline's ongoing research agenda should explore participation, representation, and democracy in America.

Positive Theory and *Voice and Equality*

JOHN H. ALDRICH *Duke University*

Voice and Equality is a major addition to our understanding of political participation and, thus, to one of the central aspects of democracy. VSB contribute, at the very minimum, a fine analysis of yet another new data set they have gathered. They build, more importantly, upon the various intellectual edifices each of the three have developed. Their theory of civic voluntarism has as its central formulation that participation "rests on three factors: resources [especially that the individual has time, money, and skills], psychological engagement with politics, and access to networks through which individuals can be recruited to political life" (p. 267). No one of these three factors is original to this study, although each has novel aspects presented here. Nonetheless, their theory is a significant advance, especially because of the comprehensive integration of the three factors. And there is the wonderful empirical documentation of each step in their reasoning, leavened by application of the best and most appropriate methods (statistical and otherwise). This book actually is what glowing reviews so often claim: must reading for scholars and sure to make required reading lists for graduate students.

VSB make a strong case for the value of cumulation in scientific explanation. The following is a set of comments in the spirit of both science and cumulativeness of knowledge. The particular question is whether this, the most recent and best attempt to understand political participation, has perhaps encountered its limits.

One of the most important contributions of science is categorization, that is, indicating which phenomena are alike, which similar and in what ways, and which are simply noncomparable. The civic voluntarism theory, like its intellectual predecessors, rests on the presumption that a large array of acts of participation deserve to be studied together as comparable acts. To be sure, views on how these acts fit together have changed over the years, from seeing them as a single dimension of participation, differing primarily in the level of demand for performing the act, to seeing them as differing in several ways and thus representing different modes of participation. VSB see participation as acts differing in three ways: in terms of the time, money, and skills required for that act; in the nature of information the act communicates; and in the "volume" of the activity and thus the extent of the pressure it exerts (see especially pp. 43–7). But in all cases, each is an act of political participation, comparable to any other such act, explainable through the same theory, in this case the theory of civic voluntarism. Moreover, here, as in preceding work, VSB find it both theoretically possible to speak of an overall level, volume, or extent of political participation in general and empirically sustainable to consider a global dimension of participation.

This theoretical stance commits the scholar to follow one path of explanation and eliminates following other paths. The most prominent alternative path in this case is to assume that each specific act of participation is comparable to and thus understandable in relation with other acts in that specific domain. For example, turnout is treated in *Voice and Equality* as (an often limiting) case of all kinds of participation. Instead, one might view turnout as a part of voting, understood as an act to stand in comparison with choice of candidate or of party. Rational choice models of participation (e.g., Riker and Ordeshook 1968, Ferejohn and Fiorina 1974) make that comparison explicit. To illustrate via a two-candidate contest, one chooses among three acts: voting for one candidate, voting for the other candidate, and voting for neither candidate. Whether one votes, therefore, depends on consideration of whom or what one would support, once in the booth, and cannot, in this theory, be understood except in relation to candidate choice. In similar fashion, Campbell and others (1960) have a chapter on turnout that employs essentially the same variables (e.g., party identification) as they employ in understanding which candidate the voter chooses. This alternative theoretical path would seek to understand participation in interest groups via the same theories and variables used to understand other aspects of interest-group politics, to understand campaign participation using theories of campaigning, and so on.

The answer to the obvious question of "which path to choose" is that there is no one "right" way to define what an act of participation compares with, theoretically. Complex phenomena, in part because they are complex, have aspects in common and aspects by which they differ. The "correct" answer, then, is that the best path to follow depends upon the object of explanation and which theory offers the best accounting of that object. The two paths I have outlined have quite different questions. *Voice and Equality* has participation and its consequences as explanandum. *The American Voter* and "The Calculus of Voting" have elections as explanandum.

One might (correctly) respond to the question of which path to choose by saying that explaining both sets of phenomena are important and useful, even if they are so for different purposes or at different times. While different in theory and explanation, they are but two different slices of the same reality. There may be, however, limits to the explanation when phenomena that are too disparate are put together in the same set. Consider, for example, writing a letter to the editor about some public policy concern. Certainly, personal resources are needed, and, certainly, there must be psychological engagement with politics. Yet, just as surely, the most potent variable would be the individual's concern about the policy and perhaps comparison to the status quo, some alternative under consideration, or some campaign platform plank. Furthermore, most of us—meaning "us" both as scholars and as citizens—would understand the letter quite differently if we knew that it came from the citizen, self-generated, or came from the citizen in response to a request from an interest group to write in support of the group's position. This candidate for "most potent explanatory variable" is quite different from the variables in the theories of turnout-as-part-of-elections noted above and, indeed, would be quite different from what we know we find in explanations of protest or informal community activity.

Consider, for another example, VSB's third explanatory factor, networks of recruitment to political activity. In their discussion of recruitment, they note that they are using this term deliberately and in contrast to "mobilization" (p. 133, n.1). They quite correctly want to emphasize the active solicitation of support by the recruiter. But both terms have valuable—and distinct—roles to play in understanding participation. Mobilization conjures images of old-fashioned, partisan campaigning; the turning out of nineteenth-century partisans likened to marching armies. Quite rightly, it makes sense to have a term specifying a relationship of current activists engaging those *already committed* to take an active role, because it now is, in fact, the time for all good men and women to come to the aid of their party. That is, it is already *their* party they are being asked to aid. It also makes sense to have a term for engaging people in the cause by convincing them that this is a cause they should support, as well as sinking the "hook" and getting them actually to support it. Recruitment to become both active and a supporter and mobilization of the inactive supporter to become an active one are distinctive enterprises and are typical of different domains of political activity, perhaps therefore better understood in the context of that domain.

A second hallmark of scientific theories is that they are designed for a single purpose: to answer "Why?" Theories of political participation in general, such as in *Voice and Equality*, illustrate both the advantages and the limits of considering all acts of participation as theoretically similar. It is both credible and convincing that these acts do, in fact, share a common explanation and that the three elements offered here (resources, psychological engagement, and networks of recruitment) are both theoretically and empirically valuable.

They may well be the three things that all kinds of participatory acts share. The limit, I believe, is that these variables are insufficient to answer the question of why someone participates. They can take you far, but only so far and not so far as to know, satisfactorily, why people participate. These variables may be (as VSB themselves develop, see especially pp. 127–31) something like necessary conditions for participation, such that in their absence participation is unlikely. Still, having even copious resources, strong psychological engagement in politics, and dense networks soliciting one's activity is not, I believe, sufficient to answer "Why did she get involved?" What is missing is a domain-specific measure of political preference, of what it is they want to achieve, or, in short, why they are participating. In particular, the individual must care about the political end sought. Why did she, for example, contribute to NARAL? The response that she is wealthy, feels efficacious, and is asked to contribute is undoubtedly an important portion of the answer. But it is also insufficient. A sufficient answer would include (at least) that she supports pro-choice policies and that she thinks NARAL is a supportable group seeking appropriate policies on abortion (and perhaps that she either seeks to maintain the status quo, fearing the alternative is far worse, or seeks, as she expects NARAL does as well, a new policy sufficiently different from the status quo to make contributions worthwhile). While what constitutes a satisfactory answer to "Why?" is at bottom a matter of judgment, I believe that resources, psychological engagement, and solicitation to the arena are valuable but incomplete.

Why incomplete? Why not just add a variable, such as "agrees with goals"? (Indeed, VSB come very close to doing so in general, and they actually do so in the specific case of abortion.) Why complicate life with many theories: of voting, of campaigns, of interest groups, and so forth, rather than a more general theory of political behavior? The latter may be found, but it will be more than a theory of who participates and who does not. Participation is a choice among a set of actions that include *how* one chooses as well as whether. It is a choice based on the ends the individual desires to achieve. If you truly do not care who wins the campaign, you will not work hard, if at all, for a candidate. Participation is, necessarily, choosing a means toward an end. Any act of participation is engagement in a complex process involving other individuals and groups acting (or not) in a wide array of ways. Campaigns, for example, involve not only voters and abstainers, not just those who write checks and those who solicit them to do so, but also the candidates, their parties and activists therein, interest groups, media personnel, and so on. VSB's emphasis on networks of recruitment opens this line of inquiry. Its consequence, I believe, is that it is theoretically more useful to embed the turnout and campaign participation choice in a theory of campaigns and elections; interest-group activism decisions in a theory of interest-group politics; and so forth. A general theory is one that includes all actors and institutions in a domain. A general theory of all kinds of participation, instead, can provide a list of what are essentially necessary conditions, perhaps just the set of factors developed here. It is unlikely to tell us, in a sufficiently satisfactory final answer, why someone participates. Only theories that involve the wider array of actors, goals, and institutions can finally answer why.

Where, then, does this leave the theory of civic voluntarism, this newest theory of participation? Instead of a complete theory of any one thing, it is, I believe, an incomplete—but extremely valuable—part of many theories (at least assuming we are still far from a general theory of politics in a republican democracy!). It will be a component of all kinds of theories of political behavior, it will help us understand who is engaged in each political domain, but it will be only one kind of choice that will, in time, be explained simultaneously within the institutional setting that specifies the domains of elections, community action, interest-group politics, and the like.

Normative Theory and *Voice and Equality*

JANE MANSBRIDGE *Harvard University*

Voice and Equality details how income, education, churchgoing, and other advantages generate the vastly unequal amounts of money, somewhat unequal civic skills, and relatively equal amounts of time, with which citizens exert highly unequal amounts of pressure in the political process. The book should have a permanent effect on the way political scientists think about the political process. And if lawmakers paid attention to political science, then it should have an immediate effect on campaign finance reform.

Verba, Nie, and Kim (1978) revealed that political systems with specifically working-class parties (like many in Europe) are most able to mobilize working-class citizens. Ferguson (1995) and Block (1987) have made an excellent case for the heavy influence of corporate interests on government policy both through direct monetary contributions and through the threat of capital flight. *Voice and Equality*, going farther, documents how even in the realm of citizen participation liberal democracies fail to live up to the norm of equal responsiveness to the interests of each citizen.

The theoretical apparatus that VSB introduce is as important as their groundbreaking findings. After this book, political scientists will surely begin to focus more on the "representative system" as a whole, going beyond the electoral connection to add interest groups, the media, and now the active citizenry as explicit avenues of representation. VSB depict active citizens as what I would call "informal representatives" for the less active citizens. They investigate, in effect, the representative/constituency congruence for this form of representation, showing with their measures of "representational distortion" how this congruence is often low.

VSB can address this distortion adequately for the first time only because the innovative nature of their survey design lets them distill from a huge national cross-section (N = 15,053) a nested representative sample of political activists sufficiently large (N = 2,517, along with their African American and Latino oversample) to enable multivariate analysis. This costly but highly effective design allows VSB to identify and survey in depth activists of all types, a strategy that differs dramatically from the usual one of identifying a priori a population of activists (e.g., convention delegates or members of political interest groups) and comparing them to cross-sectional samples of the U.S. population. As the first in-depth survey of a representative sample of activists, this work can now plumb the embedded problems of representational distortion.

Why should we care if citizens participate unequally? The self-protective, or adversarial, theory of democracy gives one answer: Less participant citizens have a reduced capacity to protect their own interests in competition with others and to receive favorable treatment from the government. The competing "participatory" theory of democracy, as expressed in such works as Kaufman (1960), Pateman (1970), and Barber (1984), gives another: Less participant citizens have a reduced capacity to develop their faculties through joining with others in deliberating on and forging a common good, a process that can clarify their conceptions of their interests, enlarge those conceptions by encouraging them to make the good of others and the whole their own, generate greater feelings of political efficacy, and ultimately benefit the larger society by anchoring it in a citizenry clearer about its interests and reponsive to the claims of justice and the public weal.

VSB explicitly recognize and credit (pp. 10–12, 163 n.1, 500–8, 528–9), but have chosen not to focus on, this second democratic tradition, which values participation for its educative effects on the citizen. Investigating these effects would have cost far more than this already extremely expensive study. It would require an experimental design that measures in one group, before and after the experience of participation, the characteristics one expects participation to promote, and then compares that to a comparable group, measured at both times, which has not had the experience of participation. The cost of carrying out such an experiment with a sufficient number of cases for the results, if any, to be persuasive has always been prohibitive. Scores—perhaps hundreds—of studies have shown correlations between political participation and the personal characteristics, such as political efficacy, that we presume participation produces (cf., in *Voice and Equality*, the correlations between participation and both information and tolerance). But self-selection could always, in theory, account for the entire effect. Only an experimental design could establish a causal instead of a merely associational relation, and the few experimental studies we have are highly time-limited, producing somewhat surface results. (It is partly because of the financial costs of following up empirically on the theoretical work of Kaufman, Pateman, Barber, and others that this important strand in democratic theorizing never achieved the next step in the usually fruitful pattern of practice-thought-practice. The other major cause was, of course, the declining number of participatory organizations in the less progressive political climate of the 1980s and 1990s.)

Within the adversarial tradition, which has always had most salience to empirical political scientists in the United States, VSB make the sophisticated normative argument that the democratic value of equal consideration generates a norm of the equal protection of interests that is undermined by the unequal participatory patterns they uncover. To the normative objection that these levels of participation are freely chosen, they have two responses. First, their evidence regarding the great effects of resources on participation (e.g., the huge effects of income on monetary contributions, and the effects of parents' education on civic skills) suggests strongly that differential resources rather than differential motivations are a main cause of differential rates of participation. Even political attention is, they argue, not simply a matter of taste but partly the result of fewer political resources. Second, they point out that the norm of equal protection of interests has an independent and strong standing among American political values, a standing probably greater than, though sometimes in conflict with, the norm of a political process open to as much influence as one's wealth and other resources allow one to exert.

Following the adversarial tradition, VSB define "politics" as influencing formal policymakers (pp. 38–43), not as, for example, citizens talking among themselves in ways that change their conceptions of their interests. They define a "politically relevant" characteristic as one "deemed noteworthy by a public official at whom activity is targeted" (p. 14), that is, being an attribute of groups "arranged around some of the enduring fault lines of political conflict" (p. 25), not as an attribute of citizens that would lead them to engage in a search for the common good.

The adversarial tradition tends also to see the primary political interests of citizens as based in self-interest. Here, VSB depart sharply from that tradition, questioning active citizens on their motivation for activity and finding that large percentages (from 93% for voting to 40% for particularized contacting) give some form of "civic gratification" ("my duty as a citizen," "I am the kind of person who does my share," "the chance to make the community or nation a better place to live") as one of their reasons, and fairly high percentages (from 81% for contributions to an issue organization to 9% for particularized contacting) give some form of civic gratification as their *only* reason for acting. VSB agree that reported motivations are liable both to reconstruction for post-hoc plausibility and to inflation for social desirability, but they point out that the response patterns vary by forms of participation in ways which make it unlikely that social desirability accounts for the entire phenomenon. They also forgo describing civic motivations as "expressive," probably having picked up from the philosophical literature the point that motivations deriving from duty are conceptually distinct from, even opposed to, motivations deriving from desire.

In the adversarial model of democracy, the relevant normative rule is that conflicting interests should be represented in proportion to the numbers of interest-bearers in the population. VSB advance both normative theory and empirical knowledge in this tradition by considering in depth for the first time the role of citizen as representative, and by demonstrating to devastating effect that both descriptively and substantively the existing participatory system in the United States results in major representational distortions.

Practical Politics and *Voice and Equality*

JENNIFER L. HOCHSCHILD *Princeton University*

This is a difficult book to review for at least two reasons. The first is its excellence. It is simply hard to find much to criticize, and it is almost impossible to find a flaw really worth sinking one's teeth into. No sooner did I discover a weak spot than it was dealt with sensibly and usually persuasively. Is the model in danger of treating correlation as causation, or of assuming that the causal arrow runs in one direction rather than the other? VSB use two distinct statistical techniques (OLS regression and two-stage least-squares) on the same issues to deal with the latter problem, and they explain why correlation suffices for their purposes to address the former. Does the question about abortion on their survey create more polarization than "actually" exists in the public, at least according to other surveys? Yes, they report, it probably does, but that potential flaw makes possible a fascinating discussion of the effect of intense, single-issue politics. Should political scientists focus on the role of institutions rather than on traditional behavioral variables? VSB report a simple, neat, and effective way to distinguish variation across individuals from variation across institutional contexts. And so on; this book shows over and over the salutary effects of smart people working very hard for a long time on a difficult problem. VSB have produced a set of results and an argument that are subtle, powerful, significant, and persuasive.

Voice and Equality also is hard to review because its mode of presentation is that of sweet reasonableness. Footnotes bend over backward to show why apparent differences between the findings in this book and others are not really disagreements, or are most likely due to alternative choice of methods. VSB carefully report points at which their own results are tentative, or follow their analysis of the deep problems of democratic participation with a discussion of its sometimes surprising virtues. Even when they devastatingly demonstrate the shortcomings of others' analyses—pointing out, for example, how wrong it is to generalize about "political participation" from voting studies—they do so in a way that invites those others to join their new exploration rather than retreat into truculent defense. So, although this book provides an array of findings that shake up much of what we thought we knew about political participation, it does not do so in a way that makes it easy, or even appropriate, to mount a vehement critique.

So what is a poor reviewer to do? In my case, I will compare the arguments in *Voice and Equality* to arguments elsewhere in an effort either to create some useful controversy or to draw out more sharply than do VSB some of the startling implications of their analysis.

Consider, to begin with, the book's discussion of the relationship between the views of politically inactive citizens and activists. Various analysts (Skerry 1993, Lichter 1985, Mansbridge 1986) have argued that leaders, at least of identity-based groups, are even more liberal than are their liberal followers. Yet, VSB find that, although 28% of the poor hold liberal attitudes on economic issues, only 21% of poor activists do (p. 215). Among African Americans, although 22% strongly favor government help for blacks, only 17% of activists hold the same view; among Latinos, even more (24%) favor government help for Latinos, but fewer than half that many activists (11%) concur (p. 240). (The latter two findings may be partly explained by the fact that activists are less likely to receive means-tested benefits and have higher incomes than nonactivists of their own race or ethnicity. The results hold for Latinos, however, even when those of Cuban origin are excluded from the analysis [p. 241].) Thus, "the participatory representation of African-Americans and Latinos is diminished in two ways: they are less likely to be active, and group activists are less likely to represent the distinctive policy positions and needs of the group" (p. 241).

These are explosive results. They imply a (growing?) class disparity within the black and Latino communities and perhaps the conservatism of the would-be upwardly mobile within the community of the poor. They also imply, although it is never stated, that previous research is wrong or at best is right about an unrepresentative group of activists. More generally, these results reinforce *Voice and Equality*'s broadest argument, that conservatives (and the wealthy, but that is a separate point) exercise a voice disproportionate to their share of the U.S. population.

Another set of findings also challenges what we thought we knew about identity-based politics. Broadly speaking, after controlling for various personal and institutional resources and capacities, race and ethnicity play no role—and gender and parental class play only a small role—in determining whether Americans are politically active. This is not to say, as VSB point out, that people of different races, genders, or classes participate at the same level. Indeed, quite the contrary. It is to say that ascriptive traits matter only indirectly, by determining citizens' education, family income, political information and interest, and other factors that do directly determine participation (pp. 440–2). In short, identity per se is irrelevant to political activism.

More pointedly, VSB tried hard to find effects of group consciousness on levels of political activity or engagement but without result; there simply was no relationship between political participation and racial or ethnic identity among African Americans and Latinos. In typical fashion, VSB find this "puzzling" outcome "consistent with recent scholarship on the political behavior of African-Americans" (pp. 355–6). It may well accord with the research that *Voice and Equality* cites, but it seems at odds with the rise in nationalism among (especially affluent) African Americans reported in recent surveys and demonstrated in such acts as the Million Man March and skepticism among blacks of Colin Powell as too attuned to white interests (Dawson 1994a, 1994b; Morris 1996; Hochschild 1995). Have identity politics sharpened and hardened in the half-decade since VSB conducted their survey? Did the civility and smoothness so evident in *Voice and Equality* itself rub off onto the survey that provides its evidence, so that blacks who in some circumstances might be vehemently nationalist were here sweetly reasonable? We do not know, and the discrepancy remains as important as it is "puzzling."

If we step back from questions about how and why particular groups of Americans participate to look at broad patterns of activism, other issues arise. VSB offer helpful observations on the question of whether so many Americans abstain from politics out of ignorance, fear of reprisal, complacence, alienation, or lack of opportunity. Their answer is, roughly, all of the above, with the probable exception of fear, for which they found almost no evidence.

As always, their discussion is sensible and well backed by careful data analysis. It was in this facet of the book, however, that I felt the greatest frustration about VSB's single-minded focus on survey results as their evidentiary base. For example, research using focus groups (Conover, Leonard, and Searing 1993; Conover 1996) does a much better job of showing Americans' robust sense of rights and anemic sense of civic responsibilities than do VSB. How are we to reconcile *Voice and Equality*'s report that many Americans find great gratification in political activism with the (Conover et al. 1993) finding that, for most respondents, " 'I don't have to work for the community to maintain my respect' "? And how are we to reconcile VSB's conclusion that few Americans fear to participate with intensive interviews in which even a vigorous and self-confident respondent continually censored herself out of concern that " 'I'm saying too much that I'm not supposed to be saying' " (Hochschild 1981)?

An easy answer is that the qualitative findings I am quoting simply exemplify the fact that a statistical generalization always has exceptions. Most citizens include political activism in their amour propre, even if the members of a particular focus group denigrate it; most citizens do not fear to participate, even if a few do. But that answer elides the possibility that even the best survey is much better at recording why people do things that they are proud of and can explain easily than at probing why people shrink in fear or laziness or simply operate in a miasma of slightly embarrassed uncertainty. Surveyors do worry about responses based on social desirability and are beginning to probe what lies behind responses of "don't know" or "no answer". But the imbalance between what surveys can and cannot reveal may be an insoluble substantive problem, not a matter of better question wording or more sophisticated statistical techniques. Perhaps, in the end, a survey is simply an insufficient instrument for answering VSB's basic question of "why people do *not* take part in politics" (p. 15, emphasis in original).

One of the most fascinating threads running through *Voice and Equality* is the distinction between voting and other forms of political participation. VSB show that voting is more equally distributed and conveys less substantive information to policymakers than do all other forms of political activity. Voters are motivated by different concerns and driven by a different set of resources and capacities than are other activists. They are recruited differently. In short, "on every dimension along which we consider participatory acts, voting is *sui generis*" (p. 24).

I have no criticisms of how VSB demonstrated and explained this distinction, and I applaud their insistence that voting neither stands in for other forms of activism analytically nor matters to the exclusion of other forms of activism substantively. My concern is that they get so carried away by the importance of their message about the uniqueness of voting as a form of political participation that they lose track of its centrality as a political act. After all, some lavishly funded candidates or referenda lose at the ballot box, and some candidates win elections simply because they appeal to more voters than do their opponents. Yet, fewer than half the eligible citizens vote, and the more effect their vote could have (that is, in local rather than state or national elections), the less their inclination to go to the polls. Just as seriously, the proportion of poorly educated adults who vote has declined over the past three decades (Farley 1992), and "the percentage of blacks who . . . cast ballots was greater in the Goldwater-Johnson election of 1964 . . . than in the Dukakis-Bush contest of 1988" (Rosenstone and Hansen 1993). The proportion of adults with school-aged children is declining as a share of voters, and parents of color with children in inner-city public schools do not cast anywhere near their potential share of votes in urban elections—and this in an era when schools are financed by local and state taxing decisions, and educational outcomes matter more for life chances than ever before. In the end, these great and perhaps growing discrepancies between voters and nonvoters may matter more than all the discrepancies between political activists and nonactivists that *Voice and Equality* analyzes so well. VSB do point out that "voting is . . . , in a profound sense, the most basic citizen act" (p. 9). But the thrust of their argument is that, luckily, it is *more* equally distributed than all other forms of political activism. And the energy in their analysis lies in their dissection of all those other forms and the inequalities therein. From the perspective of intellectual exploration, that is the correct focus for their energy; from the perspective of democratic politics, it may not be.

That brings me to my, and the book's, central point: "The public's voice is often loud, sometimes clear, but rarely [on the basis of their evidence, I would say "never"] equal" (p. 509). Monetary contributions to campaigns lie at the opposite end of several continua from voting. They are by far the least equally distributed form of political activity, and they are often accompanied by very informative and urgent (that is, loud and clear) messages to policymakers. Unlike other forms of political activism, all it takes to make a campaign contribution is a lot of money—but a high income is, unfortunately, necessary as well as sufficient. Since the affluent give most of the money and are more conservative than the rest of the population, conservative views dominate within and even distort the arena of citizens' political participation. This is the best brief for campaign finance reform that I have read, although VSB refrain from drawing any policy implication from their analysis.

Here, finally, is where VSB brilliantly illuminate one of the great ironies of U.S. politics. The only force that has any hope of offsetting the ability of the (conservative) rich to dominate political activity through the use of their money is the skills that the poor develop through participation in organized religious activities. As VSB point out too often, once one learns how to manage a church rummage sale, one can use those skills to manage a school board candidacy. In short, David's church-based political capacity can sometimes win out over Goliath's income-based political resources. But in the end, from a substantive policy perspective, there turns out to be no real contest between David and Goliath because church-goers are just as conservative as the affluent. Thus, campaign contributions (from the rich) distort in a conservative direction the relationship between citizens' underlying economic attitudes and the expression of economic attitudes in the political arena, but so do the political skills developed through church attendance (by the poor). Similarly, Latino church-goers distort the transmission of Latinos' views about government assistance in a conservative direction almost as much as do wealthy Latino contributors. Although it does not do so explicitly, *Voice and Equality* concludes with a slightly new answer to the old question "why is there no socialism in the United States?" Because both poor and wealthy citizens who are unusually active in politics do not want it, even if the rest of the population—rich, poor, and in between, but politically inert—may.

Reply to Reviews

SIDNEY VERBA, KAY L. SCHLOZMAN, and HENRY E. BRADY

> Your shiny, priceless book, upon this date,
> Goes forth to meet its anti-climactic fate. . . .
> The worst reviews of all are those that sigh,
> "Another book is here. Hello. Bye-bye."
> —John Updike[1]

We appreciate that this review symposium has saved us from the anticlimactic "Hello. Bye-Bye" that all authors fear. We are grateful for the words of appreciation, for thoughtful critiques from several perspectives, and for the opportunity to think once again about a subject to which we have given thought for more years than we care to count.

Each of these reviews treats several aspects of *Voice and Equality*, and each approaches our book from a different point of view. John Aldrich focuses on our attempt to develop a theory to explain citizen participation. Jane Mansbridge emphasizes the relationship of our work to an understanding of how things should work in a democracy. And Jennifer Hochschild considers our argument in terms of how things do work in American politics. Since we had hoped that our work would have something to say about all three—citizen behavior, politics in a democracy, and politics in America—we are delighted to see this triad of concerns represented in the reviews. Interestingly, all three reviews make clear the limitations on what we can learn about participation by using sample surveys. On the basis of survey data we were able to extend significantly our understanding of the roots of political participation. Nonetheless, each of the reviewers makes a compelling case that limitations of our data, in fact of any survey data, leave significant gaps in that understanding.

Aldrich frames his review in terms of the way that the decision to become active is understood and explained. As he notes, we focus primarily on the decision of whether or not to participate and give less attention to choices about the "direction" of participation. He draws a contrast between our intellectual project, which is to explain a decision to attend a protest or to make a campaign contribution, and that of *The American Voter,* which is to explain votes for particular presidential candidates and, thus, electoral results. He is quite correct that, by and large, the factors in our model of the sources of political activity—resources such as income or civic skills, motivations like political interest or efficacy, and recruitment by others—are "preference-neutral." That is, they can lead to activity on any side of any issue. A fuller account of participation, he argues, would pay attention to "domain-specific measures of political preference." Had we done so, he contends, our study would have been more useful in explaining political outcomes.

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[1] *Boston Globe*, April 19, 1985, Reprinted by permission of the author.

We believe that we have not ignored measures of preference quite as completely as Aldrich might imply and that our findings shed considerable light on the way the participatory process brings into politics particular preferences and excludes others. As is common in studies of political behavior, we present descriptive information on the differences between activists and inactivists in their policy preferences. Yet, we also provide a rich account, based on open-ended questions, of the substance of participation—the issue or problem, if there was one, behind each participatory act. These data were novel in conception and time consuming to collect and code, but they provided surprising intellectual payoff: demonstrating both that modes of participation vary in terms of the issue messages they carry to public officials and that members of groups with low rates of activity have distinctive sets of policy concerns. The analysis, thus, makes clear the issue bias inherent in the participatory process. Greater participatory equality would yield a different mix of policy concerns being expressed to political leaders.

Furthermore, our explanatory model does accommodate consideration of issue preferences and has something significant to say not only directly about who becomes active but also indirectly about the outcomes of that participation. Most of the factors that facilitate participation are preference-neutral. As our analysis makes clear, however, these factors are distributed unequally across the population and therefore work in such a way as to have a directional effect. Those who are well endowed with participatory factors have different political needs and preferences from those whose stockpile of participatory factors is smaller. This circumstance has inevitable consequences for both who gets involved and what the government hears.

We also demonstrate (chapter 14) that, all else equal, intense views on particular issues—what we call "issue engagements"—also operate as a participatory factor; that is, they have an independent effect in raising political participation. We illustrate this finding with an extended analysis showing that extreme views on the issue of abortion—whether pro-choice or pro-life, but especially pro-life—raise an individual's level of participation beyond what would be expected on the basis of resources, motivations, and recruitment. Those with extreme pro-life views are an especially interesting group. They are not particularly well endowed with the preference-neutral factors that enhance participation. Yet, they are more active than we would predict from these factors. In contrast, those with extreme pro-choice views have a larger stockpile of participatory factors but a longer agenda of participatory concerns. On average, they are more politically active than those with pro-life attitudes, but their activity is distributed across a large number of policy concerns, while those with pro-life views concentrate their participatory firepower almost exclusively on the issue of abortion. The net result is that what the government hears about this contentious issue overrepresents pro-life opinion.

We would have liked to replicate this analysis across a variety of issue areas, but the limitations of our method prevented us from doing so. The problem is not that surveys are poorly suited for dealing with the issue content of participation. Quite the contrary. As mentioned, each time a respondent reported having taken part in some way, we asked whether there was an issue or problem associated with the activity. The replies we received indicated a wide array of policy issues behind political participation. So widely dispersed were the issues cited in these open-ended responses that in 2,500 interviews only a few issues emerged for which there were enough cases for analysis. Abortion was a rare example of an issue for which there was sufficient activity to allow us to see the effect of preferences on activity. With a much larger sample we might have been able to build into our model greater concern with particular issues as a source of participation.

Aldrich's reference to the way that *The American Voter* seeks to explain not just the vote but the direction of the vote prodded us to think about participatory acts from still another perspective. His comments helped us recognize that political acts vary along a continuum we had never before considered: They differ in the extent to which a generalized predisposition to act can precede a commitment to the direction of the activity. It makes perfectly good sense to say, as many people did in the waning days of the 1980 presidential campaign, "I just can't decide whether to vote for Carter or Reagan next week." The decision to vote has been made; the direction of the vote is in doubt. The analogous statement about attending a demonstration about abortion, "I just can't decide whether to go to the pro-life demonstration in Washington next week or wait and go to the pro-choice demonstration next month," sounds considerably less reasonable. When it comes to voting, the decision to go to the polls can easily come before the decision about vote preferences. For other forms of activity, such as contacting a public official or attending a protest, preferences are likely to precede the decision to take part.[2]

Pointing out that we cannot treat all decisions about preference-based political activity as analogous to choices made in the polling booth suggests that we should amplify briefly our treatment of voting. Hochschild cites our repeated demonstration that the vote is different from other political acts, but she wonders—as have other readers of our book—whether, in focusing on the uniqueness of the vote, we give it too short shrift. If so, the lack of emphasis was unintentional. We stand by the contention that, as a form of political activity, the vote is sui generis and that political scientists err when they generalize what they know about the vote to other modes of participation. Nonetheless, as we make clear at the beginning of our book (p. 9), we agree fully that the vote is central to democracy. The aggregate of citizen votes determines who runs the country and gives to citizens their main mechanism for holding leaders accountable. If we imply otherwise by our stress on other forms of activity and their difference from voting, the suggestion is inadvertent.

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[2] Of course, we are omitting at this point a concern for the selective benefits that might accompany activity. We know perfectly well that activity can be undertaken almost irrespective of its policy direction if the selective benefits are sufficiently compelling. For example, when the boss asks for a campaign donation, it may be almost irrelevant which candidate or party is the recipient.

Mansbridge places our work in the context of democratic theory. As she notes, because our emphasis upon participatory inequalities stems from a concern about their potential to jeopardize equal protection of interests, we fall squarely in the adversarial tradition of democratic theory. This approach contrasts with the participatory democratic tradition, which, as she puts it, "values participation for the educative effects it has on the citizen." The latter tradition is important and worthy of investigation. As Mansbridge notes, to trace the educational effect of participation would require an experimental design. This is an even more challenging research project than ours, which we found challenging enough.

It is interesting to note how these two ways of thinking about democratic participation relate to one another and where our analysis fits in. In the adversarial tradition, the preferences and needs on behalf of which citizens act are a fixed point of departure, not an object of inquiry. In the participatory tradition, informed citizens deliberate "among themselves in ways that change their conceptions of their interests," thus rendering their preferences less self-interested and more community oriented. Like analysts in the adversarial tradition, we take citizen needs and preferences as given and consider the extent to which these receive equal expression through the participatory process. Nevertheless, as Mansbridge notes, our analysis demonstrates that a great deal of activity cannot be characterized as self-interested in any narrow sense. The policy issues associated with participation frequently are not obviously self-interested. Moreover, respondents reported "civic" gratifications, that is, participatory rewards deriving from having done one's share or serving the community. In short, although we ignored the processes by which political goals are defined, we did not find political participation to be uniformly self-interested.

By demonstrating that educational attainment creates not only enlightened participation but also stratified participation, our analysis also makes clear how these two traditions may bump up against one another. On the one hand, formal education leaves residues—political interest and information, tolerance, a sense of civic duty—that make possible the kinds of deliberative processes intrinsic to educative participation. On the other, formal education also transmits advantage across the generations and sorts people into jobs that produce differential incomes and civic skills and, thus, fosters unequal political activity. In short, education plays a critical role in generating both participatory democracy and political inequality.

Bringing our analysis back to the real world of American politics, Hoschschild questions our finding that group consciousness does not act as an independent force in raising participation. We share her skepticism. Although we looked hard for the effect of group consciousness (pp. 355–6), in not one of a variety of specifications did this factor operate on its own to enhance activity, a finding that seems at variance with the current emphasis on identity issues in the politics of race and ethnicity. Quite possibly, a dog that did not bark in our data barks, and bites, in contemporary politics. Hochschild raises the quite reasonable possibility that our method, the sample survey, is simply not the most appropriate one for exploring the meaning of group consciousness and its effect on activity. It is also possible that group consciousness had more potency as a mobilizing factor during the height of the civil rights movement, which would explain why Verba and Nie (1972) report it to be a significant participatory factor in *Participation in America*, a study based on data collected in 1967. A final possibility returns us to our earlier discussion of issue engagements: attitudes or beliefs that have an effect on participation beyond what would be expected on the basis of the other factors in the civic voluntarism model—resources, motivations, and recruitment. Our analysis demonstrated both that the factors explaining activity within the population as a whole operate similarly for Latinos and African Americans (Table 15.9, p. 447) and that minority and civil rights issues figure much more importantly in the agenda of participatory concerns of Latinos and African Americans than of Anglo-Whites (Table 8.6, p. 248). Together, these findings suggest that group consciousness need not act as an issue engagement, that is, it need not exercise independent influence in raising participation, in order for minority activity to be an important vehicle bringing identity issues into U.S. politics. Though group consciousness may not, on its own, enhance activity, it may have a profound effect on the issues about which those who are group conscious choose to be active. At this point, we can do no more than conclude that Hochschild has raised an important issue, and we suggest further research.

Mansbridge and Hochschild both stress that our book underlines the inequality of political voice in the United States, an inequality that we believe has serious consequences for "who gets what" in American politics. This brings up an issue raised by other readers, who suggest we imply that citizen participation is the only determinative factor in the policy process. In fact, we argue exactly the opposite. (See, for example, p. 526). Nevertheless, what public officials hear through the medium of participation is surely one of the multiple factors that influence policy outcomes, and our data make clear that certain politically relevant needs and preferences are expressed especially loudly and clearly—and others not at all—through citizen activity. The increasing role played by financial contributions must inevitably exacerbate the inequality of voice.

In the preface to *Voice and Equality*, written shortly after the 1994 elections, we thanked both President Clinton and House Speaker Gingrich for behaving in ways so consistent with our analysis. As we write this essay in the aftermath of the 1996 election, we have no reason to diminish our gratitude to the parties and the candidates who took part. The unstated assumption during the campaign seemed to be that, while policymakers could not ride roughshod over the interests of groups ranging from senior citizens to tobacco manufacturers to veterans without expecting an outcry in return, certain groups could be ignored with impunity. The needs and desires of inactive publics with an obvious stake in government policy—welfare recipients are the most obvious example—were simply not part of the campaign discourse.

All three reviewers indicate that this is the latest but not the last word on the subject of citizen participation and make cogent suggestions about how we might extend the boundaries of what we know about it. More work, we believe, can be done using surveys—adding a longitudinal dimension or collecting additional contextual information. As the reviewers suggest, other methods would also have payoff: for example, in-depth community studies, ethnographic research on how churches and other organizations facilitate and encourage participation, or studies of how participatory messages are received and used by policymakers. Moreover, all three reviews call for a more disaggregated and contextual approach. Aldrich suggests a focus on different domains of activity, such as voting or campaigning or protesting. Mansbridge calls for a normative theory of participation that would link our concern for the distributive effects of participation with its educative effects. And Hochschild stresses group-specific factors, such as identity and consciousness that affect different groups. These are useful suggestions that would add to our understanding of participation, representation, and democracy.

Thus, we can end our reply with the theme—perennial in academic publications—of the need for further research. We are delighted that our work has engendered a constructive dialogue and thank all three reviewers for their thoughtful remarks. And we look forward to the research that will enhance what we have done, correct it, and inevitably—but, we hope, not too soon—supersede it.

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BOOK REVIEWS

Political Theory

States of Injury, Power and Freedom in Late Modernity. By Wendy Brown. Princeton: Princeton University Press, 1995. 196p. $39.50 cloth, $12.95 paper.

Solidarity of Strangers, Feminism after Identity Politics. By Jodi Dean. Berkeley: University of California Press, 1996. 220p. $40.00 cloth, $16.00 paper.

Nancy S. Love, *Pennsylvania State University*

The relationship between particular identities and universal rights is a continuing problem for activists and theorists committed to a feminist and postmodern democratic politics. In *States of Injury*, Wendy Brown undertakes a critical analysis of late modern state power to reveal how identity politics translated into rights claims often reinscribes the oppression it would overcome. Her approach is influenced by Foucault but amended by Marxian, Nietzschean, and Weberian insights. She argues that Foucault's genealogies of disciplinary power serve to displace and by implication diminish the power of the state. (Foucault's work on governmentality, which has received less scholarly attention, is, as she notes, an exception.) Following Foucault's lead, many progressive intellectuals turned away from the state just as it consolidated a distinctive form of late modern power. The result was a variety of all too easily reappropriated assertions of freedom. Among them Brown includes: progressive liberalism and its twin sister, bourgeois feminism; radical democracy in the guise of protest groups; therapeutic discourses of the self-help movement; and the more specific discourses of feminists against pornography and critical legal theorists.

In Brown's analysis, Weber and Marx complement Foucault by offering cautionary tales for attempts to institutionalize rights in law. All too often rights claims reinstall "identity as injury," that is, to gain redress through rights is also to depoliticize the conditions of a group's victimization, to construct its members' identity as victims as natural. According to Brown, rights claims are themselves effects of an always, already "politicized desire." For example, Catherine MacKinnon's attack on pornography as a violation of equal protection refigures women as victims and heterosexism as natural. Patricia Williams's defense of rights as markers of boundaries removes racist constructions of black women's sexuality from public scrutiny.

Not surprisingly, Brown places rights "among the cruelest social objects of desire" and the most devious mechanisms of social control (p. 128). She draws on Nietzsche to argue that "North Atlantic feminists" need to develop a politics free from "ressentiment." A reactive feminism of moral reproach, often accompanied by epistemological foundationalism and ontological essentialism, is symptomatic of slave morality. Brown asks those who seek rights to shift their politics from a focus on "I am" to "I want this for us," from past wrongs to desired future (p. 75). This results in an agonistic politics of postmodern judgment, a master morality of sorts. Brown insists, however, it is a less individualist politics than Nietzsche's and one which combines feminist and postmodern political spaces.

Although Brown's political theoretical critiques—these are her stated purpose—are simply brilliant, she says little about who appears in these new spaces. (She does distance herself from Aristotle and Arendt.) I found myself wondering how this politics would work for Williams. The obvious—and painful—problem with Brown's master/slave analogy reveals the need for more specific analyses of how "the politics of ressentiment" plays for different groups. It is, I suspect, disproportionately reinscribed by "North Atlantic [white?] feminists" and indiscriminately deployed in conservatives' attacks on "special interests." Brown's decision to treat the "particular constituents" of "identity's desire for recognition" as "specific for our time yet roughly generic for a diverse spectrum of identities" limits her portrait of identity politics as a "complex historical production" (p. 55). Perhaps only an always, already constructed "we" can judge whether "we" want rights now and where they fit into our emancipatory politics.

Jodi Dean's *Solidarity of Strangers* speaks directly to this problem. She proposes "reflective solidarity" as an alternative to the rigidity of identity politics and the universality of rights discourse. It presumes a "we" which is "without labels" and "in process," while recognizing that any "we" begins with an "us." Following bell hooks, Dean calls white feminists on the "victim" identity behind many of their appeals to an exclusionary solidarity. Women of color, Dean argues, often practice an identity politics which is not only compatible with, but also expressed through, disagreements within community. As Dean defines it, "reflective solidarity refers to a *a mutual expectation of a responsible orientation to relationship,*" (p. 29). Such responsibility is exercised by including the perspective of "a situated, hypothetical third" in political discourse. Any "we" entails the recognition of an "us" composed of "I's" who can—and do—context any universals "we" decide to deploy. For Dean, universal concepts and laws are less problematic than the public/private dichotomy they have been used to (re)draw in liberal democracies. Universal rights are associated with public protection for private identities. That protection is only won for marginalized groups, however, by exposing their identities, which rights then (re)exclude from public life. Only when privacy, defined as the public recognition of personal boundaries, replaces the private sphere will rights no longer require the exposure and exclusion of persons. Dean interprets the trajectory of rights claims by blacks, women, and/or homosexuals through three phases (assimilation, accommodation, and accountability) as progress toward this goal.

Dean's discourse ethics as "the contextual conversation of humanity" is a compelling response to the tensions between identity and universality (p. 155). Ranging from Habermas to Uttal, her defense of discourse mirrors the conversation she envisions. She does not suggest, however, where such conversations can and should occur. Brown's call for new political spaces complements Dean's argument (and vice versa). Brown sees a homology between the new man and the new state: He/It looks (and sounds?) different but acts the same. A "reformed exterior" may merely mask the duplicitous and ubiquitous forms of late modern state power. Read together, Brown's *States of Injury* and Dean's *Solidarity of Strangers* illustrate the pressing need—which Brown acknowledges—for analyses that consider how subjects are positioned and produced by states and discourses. Read alone, each study significantly advances our understanding of aspects of politics where particular identities and universal rights are currently in conflict.

War and Its Discontents: Pacifism and Quietism in the Abrahamic Traditions. Edited by J. Patout Burns. Washington, DC: Georgetown University Press, 1996. 240p. $55.00.

Jo Renee Formicola, *Seton Hall University*

War and Its Discontents purports to examine "quietism," that is, the restraint of force under specified conditions, and "pacifism," or principled rejection of all coercive force, within the traditions of Judaism and Islam. This volume actually does more, however; it provides a significantly broader overview of the scope of sociomoral issues such as war and peace, as well as violence and nonviolence, thus providing the reader with a context to better understand Abrahamic religious thought.

The intellectual discussion which spawned this volume took place in 1990 at the Center for Interreligious Dialogue at Washington University in St. Louis. Having brought together eminent scholars in the field to investigate the Judaic and Islamic views of quietism and pacifism, the book emerged from a methodology based on scholarly presentation and critical, constructive response.

This work contains a wealth of information for the student of religion and politics as well as political theory. The scholars involved in this project clearly defined and defended their particular religious approaches to conflict in the secular sphere; a fact which makes this book a critical adjunct to the exposition of the diversity of thought mandated by the nature of multiculturalism in the university today and current society as a whole.

The book is organized, at the beginning, around the Jewish interpretations of war and peace, as well as violence and nonviolence. Next, the Christian response is given. Then, the Islamic tradition is laid out and critiqued. Finally, a challenge is offered to all religious groups to influence the development of more moral and humane social policies.

Broyde and Gindler begin by presenting two excellent exegeses of the Talmud and the writings of the great rabbinical scholars, while Mirsky and Goodman look at concomitant Judaic principles from historical and philosophical perspectives. All four leave the reader with general standards on which to validate the coercive activity of the state: actions taken in the name of direct divine command; policies implemented by religiously established governments acting under revealed law; and measures taken by secular governments following revealed law.

These principles, however, are challenged by Yoder's piece, "On Not Being in Charge." His contribution serves as a transition in the dialogue and calls on religious nonelites within society to use quietism and pacificism to influence the state to protect the freedom of every religious organization to advance its own salvific mission. He believes that this kind of action will, in turn, bring about a more moral society.

The book then takes up Catholic responses by Langan and Wink, each of whom argues for nonviolence and pacifism based on the Augustinian tradition of just-war theory. About three-quarters into the volume, then, the book finally turns to a justification for violence in Islam by Sachedina. Emphasizing the quandary of the moral imperative to pursue a divinely revealed social order throughout the world, his exegesis clearly defends the religious validity of using force to spread religious truth.

The dialogue ends with a philosophical discussion of the potential for creative nonviolence in Islam by Nagler and a piece on secular pacifism by Gaffney. The entire book, then, is an attempt to educate the reader about the religious traditions of Judaism and Islam and to place their beliefs on quietism and pacifism on a continuum of sacred thinking on the critical issues of war, peace, violence, and nonviolence. Yet, the volume reflects an inherent disparity, a dynamic tension between the legalistic methodology of the Jewish and Islamic scholars, on the one hand, with the relational approach of the Christian respondents, on the other. As a result, the different perspectives reflect the Abrahamic interpretations and commitments to ontological obligations, while the Christian views encompass innovative and creative solutions to the challenges of the secular world. Thus, the chapter entitled "Is There a Tradition of Nonviolence in Islam?" by Nagler, which is the shortest but clearly the most insightful piece in the book, best raises the central but unanswered question of the dialogue: How can God ask humanity to avoid violence and ensure justice at the same time?

This is the problem that stymies the Abrahamic scholars, for the legalistic approach forces them to retreat to religious texts for justifications of violence *or* pacifism rather than seek ways to pursue the creative solutions between them. It is here that Nagler opens a window and points to the power of positive nonviolence. As preached and practiced by Mohatma Ghandi and Kahn Abdul Ghaffar Khan, positive nonviolence, according to Nagler, contains the seeds of action within both quietism and pacifism. It presents a creative way to "find another kind of power," a power in which religious groups can take part and lead. This includes, but is not limited to, education and other activities designed to transform the very nature of society.

Such creative solutions have been used increasingly by religious groups in recent years to make significant, peaceful change in the world. For example, in Latin America, Catholic lay and religious activists have established Christian-based communities whose purpose has been to empower individuals to develop and practice democracy and a qualified capitalism. As a result, both El Salvador and Nicaragua, specifically, are on the way to transforming themselves, as is much of society in the Southern Hemisphere. Clearly, the U.S. civil rights movement led by Southern Baptist ministers, the establishment of a foreign policy based on human rights led by the National Council of Catholic Bishops, and the sanctuary movement championed by the Presbyterian Church have brought about the development of a more moral domestic and foreign policy in the United States. Furthermore, the Lutheran alignments to bring down the wall between East and West Germany and the Catholic support of the trade union Solidarity in Poland have played a significant role in the demise of Communism in Eastern Europe. The Quaker conciliation of the Nigerian civil war, the role of the Anglican Church in destroying apartheid in South Africa, the activities of the Catholic Church in the Philippines and of the Congregationalists in Zimbabwe, all attest to the creative and positive nonviolent, religious solutions to problems within society. Yet, this volume seems to miss this basic point.

The flaw in this book is that it presents valuable information without considering creative breakthroughs and applications for many of the most volatile problems facing those within the Abrahamic tradition: how to resolve Biblical conflicts over territory in the Middle East, how to bring about respect for ethnic and religious diversity in the area, and how to plan for a future devoid of war as the millennium approaches. This volume is intellectually correct but without vision and relevance for the future. Nevertheless, *War and Its Discontents* is worth reading for the information it conveys, but recommendations for practical solutions clearly must come from the reader rather than the participants who gathered to discuss quietism and pacifism in the Abrahamic tradition.

Augustine and the Limits of Politics. By Jean Bethke Elshtain. Notre Dame, IN: University of Notre Dame Press, 1996. 143p. $21.95.

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Jean Elshtain might appropriately have subtitled her book "An Appreciation." She does not attempt a comprehensive presentation of Augustine's political views, nor does she engage very extensively in critical analysis of those views. Rather, her principal aim seems to have been to show that, even by standards common among late-twentieth-century American liberals, Augustine was a rather attractive and discerning figure. Thus, for example, his attitudes toward women were surprisingly fair and balanced for a man of his time; furthermore, he was not an enthusiastic supporter of the Constantinian settlement, as one might expect the stereotypical Augustine to be; and although he defended the concept of just war, he was acutely conscious of the evils inherent in war; finally, while he was not as tolerant as, say, John Stuart Mill, he was far from unhesitatingly repressive toward heretics such as the Donatists. In some very perceptive passages, Elshtain likens Augustine to one of the most widely studied and influential thinkers of our century, Hannah Arendt. The likeness is in their concepts of evil. Augustine did not see evil as glamorously demonic but rather as absence of good, something which paradoxically is really nothing. Arendt, as everyone knows, envisioned even the extreme evil which produced the Holocaust as merely "banal."

In general, Elshtain wants to show that Augustine was more moderate and more balanced than people today often suppose. This pertains particularly to the "limits of politics." As these words from the title of the book indicate, Elshtain sees Augustine as conscious—as many dominant figures in the twentieth century have not been—that there is much that politics cannot accomplish; he was "a thorn in the side of those who would cure the universe once and for all." Nonetheless, he "similarly torments cynics who disdain any project of human community, or justice, or possibility" (p. 91), for he was not preoccupied with human failure and blind to human goodness.

The result of these emphases is a book that does much to rectify the kind of continuous injustice done Augustine by the modern secular world. People who have only a passing acquaintance with Augustine, and judge him mainly by prevailing attitudes, can learn much from *Augustine and the Limits of Politics*. One can ask, however, whether Elshtain's Augustine is more like the real Augustine than is the Augustine delineated in secular prejudgments. He probably is. Yet, Elshtain's portrait is of a decidedly gentler figure than—in the mind of this reviewer, at any rate—the fiery African, with his tireless attacks on Manicheans, Donatists, Pelagians, and pagans of all sorts, really was.

To pursue this question it might be noted that Augustine was intensely concerned with the preservation of order, for order was necessary to the life and activity of the church, and it was ceaselessly threatened by human pride and selfishness. As a consequence, there is much in Augustine that foreshadows Hobbes. This is a major theme of Herbert Deane's classic study of Augustine's political doctrine (*The Political and Social Ideas of St. Augustine*, 1963), to which Elshtain makes no reference. Liberty (except for the church) and even constitutionalism were decidedly secondary considerations for Augustine. He leaned strongly toward the maxim of absolute obedience to the government. And while he was more strongly inclined to be tolerant than is generally realized, in the end he was not a defender of tolerance. Peter Brown, a leading Augustinian scholar of our day, and in general deeply sympathetic with Augustine, remarks that "Augustine may be the first theorist of the Inquisition" (*Augustine of Hippo: a Biography*, 1967, p. 240). Noteworthy, too, is Augustine's lack of concern with social reform. He was not indifferent to the sufferings of the poor or to the evils of slavery, but amending the drastic inequalities of his time, or abolishing slavery, were not matters for serious consideration.

The decisive factor in this whole matter may be Augustine's view of evil. In likening Augustine's concept of evil to Hannah Arendt's, Elshtain has in mind a doctrine Augustine developed in the course of his break with Manicheism: that evil is only a deficiency of being and in itself has no positive reality. It is therefore parasitic on being, or good, and cannot stand on its own. For Hannah Arendt, it might be said, Eichmann's "banality" was not a positive characteristic so much as a "blankness" (George Kateb's characterization), a characterlessness. But, to begin with, Arendt's hypothesis is more debatable than Elshtain acknowledges. Was Hitler banal? Was Goebbels banal? Arendt's view is surprisingly individualistic, in that it does not take into account the idea (adumbrated by Reinhold Niebuhr, among others) that there are different kinds of evil (for Niebuhr, pride and sensuality), and that these may cooperate in any massive appearance of historical evil. Thus, men like Hitler may empower banal evildoers such as Eichmann. It seems unlikely, however, that the banal evildoers could, all on their own, initiate and carry out such an act as the Nazi revolution. At some point, surely, men who are altogether more demonic must take a hand. Turning to Augustine, it is questionable, despite his doctrine of the negativity of evil, that sin was fundamentally banal. The chief sin in his eyes, after all, was pride, which in its extreme forms we find repellent and frightening but not banal. Elshtain herself, in another context, notes that Augustine was fearful of war because, however just the cause, it was apt to "stir up temptations to ravish and to devour"—not very banal activities. Evil may be a mere negation of reality and not in itself a reality. Nevertheless, Augustine was appalled by the presence in most human beings of an inclination to embrace and to call forth that negation. Hence, in his later life he devoted much attention to the eternal arrangements which God would make for the human sources of negation; that is, he outlined Hell, and he did so in rather lurid colors. Hence, too, the greatest issue of social and political life for Augustine was not constitutionalism, or tolerance, or even justice, but rather peace and order.

Minimizing the scale of evil in Augustine's mind, Elshtain neglects to mention a concept which was central in Augustine's thought, although unwelcome to modern ears, that of divine grace. For Augustine, the human tendency to negate being and the good could not be countered by human effort but only by grace. This meant that every human achievement was dependent on grace and that the entire drama of history (Augustine's philosophy of history being, of course, a distant ancestor of Hegel's and Marx's) was fashioned by grace. These are matters with which Elshtain is obviously thoroughly conversant, yet they form no part of the picture of Augustine she presents.

Given this and other omissions and emphases already mentioned, I do not recommend *Augustine and the Limits of Politics* as an introduction to Augustine's political thought. I do recommend it, however, to those already possessing a general knowledge of Augustine. It is an intelligent, warm, and well-informed discussion. And all of those who think of Augustine as a cruel and dogmatic old prelate whose time has long ago gone by should hasten to read it. They will discover, as countless generations past have discovered, that Augustine is a thinker for all circumstances and seasons.

Seedbeds of Virtue: Sources of Competence, Character, and Citizenship in American Society. Edited by Mary Ann Glendon and David Blankenhorn. Lanham, MD: Madison Books, 1995. 320p. $27.95.

Mark E. Kann, *University of Southern California*

The contributors to this volume share a consensus. They agree that Americans need to address the question of how to promote virtue and civility in individualistic, liberal-democratic societies. They agree that "the family" is the most important seedbed of people's morals and manners, it is now severely weakened, and the result is a deterioration in U.S. public life. Finally, they agree that this deterioration constitutes a crisis which must be addressed by revitalizing civil society and civic virtue.

Why is virtue an issue today? James Q. Wilson argues that Americans have been living on an "accumulated moral capital" (p. 19) that began to exhaust itself by the late nineteenth century. That was when ordinary people's sense of self-control, fairness, and patriotism encountered the extremist versions of liberty, equality, and relativism that constituted modernism. The result was that the young, impetuous, and unstable along with intellectuals began to lose sight of the virtues essential to liberty.

William A. Galston calls the relation between virtue and liberty "a tension in the heart of liberalism" (p. 38). U.S. society, a compound of shared values and individual diversity, is suffering an attenuation of virtue that creates pathologies such as widespread irresponsibility, intolerance, and pandering to public opinion, all of which weaken people's sense of connectedness. Galston categorizes and lists economic and political virtues essential to the health of a liberal democratic polity and concludes that the United States needs a rebirth of virtues such as self-restraint, adaptability, respect, and unifying leadership to repair "our tattered social fabric" (p. 57).

The foremost force behind our lost moral capital and tattered social fabric is the failing family. Nearly all contributors consider "the family" to be the key institution for forming decent human beings and conscientious citizens. With few qualifications or apologies, they define a "healthy" and "strong" family as one in which a married, heterosexual couple in a lasting relationship commit themselves to proper parenting to ensure that children grow up with good values, prosocial behavior patterns, and citizenship skills. David Popenoe argues that these stable families provide love and discipline to children and also a capacity for children to generalize affection and respect for authority beyond the family. He worries, however, that healthy, strong families have become rare, parents spend little time with children, and communities are now unfit places for children. This is "a national calamity" (p. 81).

A key aspect of this calamity is a deterioration of public life. Mary Ann Glendon suggests that families transmit the virtues essential to citizenship in democratic societies. The weakening of child-rearing families may be "our culture's most serious long-term problem" (p. 3) because we cannot rely on them to teach values needed in democratic societies. William M. Sullivan reminds us that we cannot rely on "the simplistic liberal creed to sustain the conditions of its own possibility" (p. 189) because individualism without virtue generates a sense of alienation and fragmentation that undermines the viability of democratic institutions. Fortunately, we have begun to recognize "the nurture of civic virtue as the central practical problem" (p. 191) in the modern United States.

The contributors mostly propose family-oriented solutions to this problem. Judith Martin (a.k.a. Miss Manners) wants us to revitalize family-based etiquette, "civilization's first necessity" (p. 63). Donald S. Browning asks us to learn from an African-American church that strengthens families by promoting "male responsibility" (pp. 121–2) within them. David Blankenhorn offers an array of social policies to strengthen families and thereby solve "the essential crisis of the liberal state" (p. 276).

The unifying consensus of *Seedbeds of Virtue* stems from fear, disenchantment, and optimism. The authors fear that radical individualism and materialism fuel rising divorce rates, teenage pregnancies, illegal drug usage, racial and ethnic intolerance, street violence, and other social pathologies. They are disenchanted with marketplace and welfare state solutions that often make matters worse. But they are optimistic that they have identified a "third way" to forge unity amid diversity. They emphasize family culture as a source of enduring virtues and support the family as a superb institution for mediating individual desire and the public good.

Let me raise three questions about this consensus. Is today's shortfall of virtue a crisis? Since the Revolution, intellectuals have complained that people's lack of virtue allowed selfishness, conflict, and anarchic violence to run rampant in society and invited corruption and tyranny in politics. Furthermore, they have identified selfishness, conflict, and violence as male vices that might be counteracted by virtuous republican wives and mothers. This gendered interplay of moral indignity and expansive liberty is a staple of U.S. history. The authors have not shown that today's dynamic is different or more dangerous than earlier episodes, or that the language of crisis and calamity is appropriate.

Is revitalization of families the basis for encouraging civic virtue in U.S. society? Families have been important seedbeds of virtue but not necessarily *civic* virtue. Just as Plato feared that families divide men's political loyalties, General Washington complained that militiamen returned to their families rather than dutifully fought for the duration of the war. Noting that families could compromise civic devotion, Benjamin Rush called on public schools to be the prime educators of citizen values and skills. The authors of this volume apparently are not aware that the modern United States hosts a robust character education movement which agrees that public schools should assume considerable responsibility for teaching civic virtue, public service, and good citizenship.

Finally, do we agree on which virtues need to be taught? Several authors refuse to consider virtue a contested concept. They make unsupported assertions about true human nature, the natural moral order, a natural tribalism, and the healthy family to infuse very conservative meanings into virtue. Other contributors assume a liberal consensus on general virtues such as personal responsibility, social respect, and political deliberation. Stanley Hauerwas is the sole contributor to suggest that virtue may have contradictory meanings. A Christian committed to virtue as martyrdom will oppose civic virtue as an exercise in arms (p. 203). The discussion of virtue is so blunt that both Jesse Helms and Jesse Jackson could lend their support. Perhaps that is the point.

Fighting Words: Individuals, Communities, and Liberties of Speech. By Kent Greenawalt. Princeton: Princeton University Press, 1995. 189p. $24.95.

Private Consciences and Public Reasons. By Kent Greenawalt. New York: Oxford University Press, 1995. 225p. $45.00 cloth, $19.95 paper.

Wayne V. McIntosh, *University of Maryland*

Kent Greenawalt has produced two well-written and exhaustively analytical books, both of which make significant contributions to our collective consideration of some very complex issues. In *Fighting Words*, he offers a thorough assessment of legal developments and academic debate in both the United States and Canada regarding the appropriate boundaries for freedom of speech in a liberal democracy. Similarly, in *Private Consciences and Public Reasons*, Greenawalt navigates a course through treacherous terrain of political theory regarding the appropriate drawing of lines between privately held religious beliefs (and other world views) and public deliberation and decision making in a pluralist liberal society. In both cases, his synthesis of the relevant literature and his careful take on the questions make these books quite useful.

Despite the title, *Fighting Words* is not restricted to an analysis of discourse the U.S. Supreme Court has placed outside the bounds of First Amendment protection because its "very utterance inflict[s] injury or tend[s] to incite an immediate breach of the peace" (*Chaplinsky* v. *New Hampshire*, 315 U.S. 568, 572 [1942]). Instead, Greenawalt casts his offensive speech net more broadly to consider "the kinds of speech mainly engaged in by extreme dissenters and outsiders" (p. 11). In particular, he concentrates on four especially provocative forms of expression—flag burning, hate speech, workplace harassment, and obscenity. This is not the stuff of reasoned debate, nor is it mere assertion of fact, opinion, or feeling; according to Greenawalt, it is "situation-altering . . . much more 'action' than 'expression,' " to which the "reasons for free speech hardly apply" (p. 6). Where one draws the line for protection depends upon how one strikes a balance between guardianship of individual liberties as opposed to preservation of communal civility. Greenawalt argues that U.S. courts, in interpreting the First Amendment, have provided too much protection to individual freedoms while underestimating the degree of injury to the larger community. Indeed, *Chaplinsky* has cast an incoherent shadow. The ability to inflict significant injury is spuriously correlated with an incitement to violence: "Words may hurt the defenseless more than those who are able to strike back" (p. 54).

The U.S. Supreme Court has emphasized the rights of individuals to be free from government restriction and deemphasized the harm that free speech may produce among some members of the community. There is no balancing act here. With few exceptions (e.g., commercial speech and obscenity), a government regulation directly affecting speech rights must be content neutral or face strict judicial scrutiny, a form of analysis that nearly always dooms the regulation (see, e.g., *R.A.V.* v. *City of St. Paul*, 505 U.S. 377 [1992], striking down a hate speech ordinance; *Texas* v. *Johnson*, 491 U.S. 397 [1989], invalidating a flag desecration statute).

Although Canada's constitutional history is very different, the Charter of Rights and Freedoms, adopted in 1982, empowered the Canadian Supreme Court to invalidate government restrictions on speech. Section 2 guarantees individual freedoms of "thought, belief, opinion and expression, including freedom of the press and other media of communication" (p. 13). But Section 1 calls explicitly for balancing individual rights against the communal good, subjecting expression "to such reasonable limits prescribed by laws as can be demonstrably justified in a free and democratic society" (p. 13). In recent years the Canadian Court has invalidated a range of speech restrictions, often referring precedents of its sister court to the south. But, unlike the U.S. Court, it has also weighed the communal interest in upholding statutes prohibiting hate speech (*Regina* v. *Keegstra*, 3 S.C.R. 697, [1990]) and peddling of obscenity (*Regina* v. *Butler*, 1 S.C.R. 452, [1992]), finding that such "speech can cause emotional damage of grave psychological and social consequence to members of the target group" (p. 66).

Greenawalt clearly prefers the Canadian approach (pp. 151–2). Although he notes the potential importance of the language difference in the two constitutions (p. 13), he is a bit vague on how the U.S. Supreme Court could engage in balancing individual and social interests in light of the First Amendment's commands. And he acknowledges the poor track record of the U.S. Court when it has attempted to do so (p. 123). (Also see, e.g., Rodney A. Smolla, *Free Speech in an Open Society*, 1992; Mark A. Graber, "Old Wine in New Bottles: The Constitutional Status of Unconstitutional Speech," *Vanderbilt Law Review* 48 [1995]: 349, 367; Samuel Walker, *Hate Speech: The History of an American Controversy*, 1994.) *Fighting Words* echoes many of the arguments presented by Catherine MacKinnon in *Only Words* (1993), but Greenawalt is more comprehensive in his coverage of issues. Offensive speech is a vexing problem. One person's "deep thoughts" are often another's worst nightmare. So long as U.S. courts continue to stress the sanctity of individual freedom of expression, injury to particular communities will be an inevitable side-effect. The alternative is suppression, and determining speech worthiness has proven to be no less thorny.

This conundrum connects with the issues Greenawalt addresses in the second book, *Private Consciences and Public Reasons*. Here he surveys the theoretical problem of where individual belief systems, particularly religious ones, should fit in a multicultural liberal democracy's process of public deliberation. The attempt to draw appropriate lines of demarcation between private and public is not new, and Greenawalt does an excellent job of synthesizing the arguments of a range of influential theorists (chaps. 6–10). Indeed, in some cases, he articulates the positions more clearly than the original author. He attempts to find a middle position between those who argue that deep-rooted personal beliefs are anathema to "public reason" and should be excluded and those who advocate for inclusion. Unfortunately, his intermediate thesis falls short.

Greenawalt argues that "ordinary citizens" should be free to live their lives, make decisions, engage in private discourse, and speak out on public issues on the strength of deeply held views and principles. It is essential to the continued stability of the system, however, that those same citizens exercise "self-restraint" when they assume positions in the public sphere. This is requiring a price that may seem unreasonable to some, completely impossible to others. Assuming that an individual with deep religious convictions could set aside her world view when addressing issues in a public arena, on what set of premises would she proceed? Religion has always been a significant force in U.S. political history (see Kenneth D. Wald, *Religion and Politics in the United States*, 1987), despite the wall of separation prescribed by the First Amendment. Moreover, it is not clear that all religious doctrines run counter to the principles of liberal democracy. How do we select appropriate ones from the belief menu?

Judges, more than any public officials, should exercise principles of restraint: "Asking them to try to decide exclusively, or nearly exclusively, on the basis of authoritative materials and publicly accessible reasons is not too great an imposition" (p. 149). This may be true, but it may also ask judges (and other public officials) to be disingenuous, to disguise their true private reasons with carefully crafted public statements. Could Justice Thurgood Marshall, for example, have genuinely checked his world view at the robing room door after devoting his career as an advocate to pursuing it? This does not mean that he could not be expected to be fair, but his decisions in the field of civil rights and liberties were certainly informed by deeply held convictions. More recently, Justice Antonin Scalia delivered a scathing attack on the forces of secularism, which to his thinking is disingenuously dressed as sophistry. The modern world views Christians as "fools," he remarked to a prayer breakfast sponsored by the Christian Legal Society at the Mississippi College School of Law, and "we must pray for the courage to endure the scorn of the sophisticated world" (Joan Biskupic, "Scalia Makes the Case for Christianity; Justice Proclaims Belief in Miracles," *Washington Post*, April 10, 1996. p. A1). He, for one, apparently intends to keep the faith.

These two books demonstrate the difficulty of taking on complex issues, and overall Greenawalt's presentations and arguments are better than most. Those interested in First Amendment problems will find both books very useful.

Chaos Theory in the Social Sciences. Edited by L. Douglas Kiel and Euel Elliott. Ann Arbor: University of Michigan Press, 1996. 349p. $54.50.

Courtney Brown, *Emory University*

Across many disciplines, chaos theory is currently one of the most watched subfields of the more general area of nonlinear dynamics. The social sciences have been similarly influenced by this broader interest in nonlinear thinking. This edited volume by Kiel and Elliott is an outstanding demonstration of how deeply nonlinear thinking has penetrated into some elements of the current generation of social scientists. The articles contained therein range from general discussions of chaotic dynamics as they may be manifest in social scientific data to specific area discussions relevant to political science and economics. The book is certainly worth careful study by all who are methodologically inclined. Its potential audience should be much wider, however, including (but not limited to) those who wish to understand the broader philosophical contribution nonlinear thinking is making to our understanding of how we live and organize ourselves.

The linear paradigm is dominant across all social scientific circles today. Its dominance is an historical artifact, however, not at all based on any real inherent superior value in linear thinking. In short, linearity is dominant today because linear mathematics and programming were the only feasible approaches to quantitative studies in the decades past, when slow mainframe computers were the only game in town. All that has changed. Computational speed and capacities have increased to the point that the original reasoning for the restricted use of only linear mathematics approaches can be abandoned in its entirety. It is not that we need to abandon linear mathematics; rather, the intellectual thinking that lifted linearity to its paradigmatic dominance restricts our view of the greater complexities in the reality surrounding us.

Fundamentally, human behavior is as nonlinear as are the physical and natural behaviors of the rest of the universe. Indeed, linearity is only a very narrow subset of the universal nonlinear condition. That we tend to look at ourselves through a linear computational lens in no way changes the fact that we are inherently nonlinear creatures. This is the challenging premise of *Chaos Theory in the Social Sciences.* This is a bold book. Its goal is more than to present a collection of interesting ideas. Indeed, the contributors act collectively to address one of the greatest inadequacies in general social scientific thinking today. I think they do so successfully, in a broad and audacious fashion.

While there are too many contributors to this volume for me to discuss individually, a few comments directed at specific articles are worthwhile. The introductory article by Kiel and Elliott has particular value to scholars not so technically inclined as well as to students new to quantitative approaches. Their treatment avoids complex computational algorithms. Instead, they demonstrate chaotic behavior using commonly available spreadsheet software, the same type used, say, to balance departmental budgets. McBurnett's article on spectral analysis is one of the most easily understood treatments of that subject currently in print. It is useful for anyone wanting to study frequency components in time-series data. Moreover, McBurnett demonstrates some of the techniques applicable to chaotic data using real political data. The article by Richards on aggregation effects of social scientific data with chaotic elements is as solidly clear and provocative as much of her other writing presented elsewhere. This particular piece would be of special interest to social choice theorists wanting to apply nonlinear and chaotic approaches to their own work.

In a section on chaos theory and political science, all three contributions are worth reading closely. Brown's piece is a solid appeal to the general idea of advancing nonlinear approaches to political phenomena. Saperstein's contribution addresses nonlinear and chaotic concepts in the realm of international relations. Finally, McBurnett presents a second piece which specifically draws the connection between nonlinear complexity and the evolution of public opinion. As with his other contribution, McBurnett uses real data to estimate and evaluate these chaotic dynamics. Those who want to get their feet wet in chaos theory must not miss these chapters.

The three contributors to the economics section are equally well chosen for this volume. Rosser examines the applicability of chaos theory in rationality as it is used in economics. Berry and Kim explore what has now nearly become a trademark for Berry, the senior author of this piece, the analysis of long waves in economic data using phase portrait methods. Dendrinos looks at cities as spatial chaotic attractors.

Each article in the volume makes a significant contribution to current discussions regarding chaos theory specifically, and to nonlinearity and complexity more generally. I recommend it to everyone interested in these issues. The book would certainly fit in a syllabus for a graduate class dealing with some of these philosophical issues. Moreover, the book is likely to warm up the healthy nonlinear versus linear debate that is current among methodologists.

The Art of Bargaining. By Richard Ned Lebow. Baltimore: Johns Hopkins University Press, 1996. 193p. $24.95.

I. William Zartman, *Johns Hopkins University*

Since it was named, in the eighteenth century, the field of negotiations has been largely the subject of books of proverbs—wise but often contradictory maxims, unconnected to a conceptual model, that ground the wisdom in theory and that are free of intervening variables which tell when in the process to apply the wisdom. Over the past three and a half decades, there has been an explosion of literature on the subject, some of it more proverbs, but much of it a more scientific treatment of the nature of the process, yielding derivative analysis and prescriptions for ways of overcoming the initial deadlock or conflict and achieving positive results for one or both parties.

It is therefore a bit off-putting to see someone venture into a crowded field with the opener: "I searched in vain for a basic text . . . I had to rely on my lectures to do the job. Those lectures . . . form the basis of this book" (p. ix). It is also troublesome that even the subject of the book is not very clear. The book often assiduously avoids the word *negotiation*, yet it defines *bargaining* broadly, if ambiguously, as "communication designed to promote a satisfactory exchange" (p. 1) or "a search for advantage through accommodation" (p. 9). It seems, therefore, that the subject is the large field of negotiation, rather than its tactics, as usually indicated by "bargaining," although accommodation is later identified as only one of the available strategies. Indeed, this book purports to present an alternative to accommodative approaches (termed the approach of the 1960s) and competitive approaches (termed, more curiously, the approach of the 1980s) by digging deeper into ends, interests, and utilities (p. xi). That would be interesting, but unfortunately the theme does not follow through the book, and the terms do not make it to the index.

The analysis is grounded on some basic models discussed in many works on negotiations (there are no footnotes in this book, so others' discussions do not show up): situations in which the goal is an agreement versus situations in which the goal is the best possible agreement (p. 9); and situations in which there is a bargaining range or contract zone versus situations in which there is none (p. 56). The relationship between these two pairs is not discussed.

The book spends more space on three strategies—coordination (or collaboration [p. 72] or accommodation [p. 9]), rewards, and punishments (or threats). The first, which relates to bargaining as defined and to the first situation of each pair, is given little attention (pp. 61–71). The other strategies receive much more. Various reward strategies are discussed, but their use is defined in terms of one another, and it turns out they can be used at any time, albeit with different implications. Various applications and implications of punishment (threats) are discussed, although with no particular reference to the initial typologies.

Asymmetries of means are also discussed in connection with punishments and then all over again in relation to leverage. If there is any conceptual key, it is the notion of alternatives or security point (asymmetries of ends). Indeed, much of the advice boils down to the awareness of development of alternatives, an important element in negotiation. Yet, even this notion is not conceptualized nor are its strategies related to types of negotiations.

Throughout the discussion, stories and illustrations are included; the most sustained one concerns the author's purchase of an automobile, much of the point of which relates to the need for good research and the development of alternatives. But the story is not the best for negotiation (or bargaining) since it involves choice among alternatives more than communication for satisfactory exchange or advantage through accommodation. Perhaps the largest hole in the process, however, is the absence of any discussion on closure, on how and when to obtain it; even the end of the auto purchase is left up to the reader to surmise.

Probably the most original emphasis of the book is the focus in Part 3 on the aftermath of bargaining or the beginning of bargaining with the home team—ratification, implementation, and clarification. As in the rest of the book, the points are sound, if not new or conceptualized; the basic positive-sum nature of negotiation finally appears: Bargains that are to the advantage of both sides have a better chance of being kept (p. 152). These chapters, like the others, end with checklists of questions to guide practice. Back to the book of proverbs.

This book makes many good points, and that is the end of it. As a coherent, comprehensive treatment of its subject bringing new analysis and new insights it falls short. For a classic, read deCallières, *On the Manner of Negotiating with Princes* (1716/1963); for good conceptual modern treatments, read Lax and Sebenius, *The Manager as Negotiator* (1986), or Mastenbroek, *Negotiate* (1989), or Pruitt and Carnevale, *Negotiation in Social Conflict* (1993), or others. There *are* good basic texts.

Revolt against Modernity: Leo Strauss, Eric Voegelin and the Search for Political Order. By Ted V. McAllister. Lawrence: University Press of Kansas, 1996. 323p. $29.95.

Steven R. McCarl, *University of Denver*

McAllister contends that, according to Strauss, political questions are always moral and religious because laws emerge out of authoritative opinion about good and evil, right and wrong, and such opinions depend upon "some myth about the nature of reality." Because modern liberal democracy entails deep skepticism concerning public truth about the nature of reality, it undermines political and moral life. By resurrecting political philosophy Strauss "hoped to reinvigorate the search for knowledge of transhistorical truths in order to secure a political and moral center" (p. 220).

As a Jewish philosopher, Strauss, however, seems to be caught between the commands of the law (Jerusalem/faith/revelation) and the philosophical quest to replace opinion with knowledge (Athens/reason). But as McAllister notes, Strauss maintains that all alleged refutations of revelation presuppose unbelief in revelation, and all alleged refutations of philosophy presuppose faith in revelation. In the end philosophy is like revelation in that it depends upon an unevident premise—an act of faith.

But even if it is granted that philosophy entails an act of faith, the tension between revelation and reason is not changed. The law of the Torah is still fundamentally different from the law of modern liberalism; it cannot provide the individualism (whether Lockean or Kantian) that constitutes the essential character of the modern liberal state. Modern liberal individualism in practice cannot guarantee a public order based on a commonly accepted transcendental order—a situation Strauss finds objectionable ultimately, but perhaps not penultimately (Strauss does reject Spinoza, which constitutes a rejection of the liberal solution). Perhaps the issue depends upon how important it is for the philosopher to have a protected—though generally inconsequential—place, which it must be granted liberalism provides.

For Voegelin modern liberalism reveals that "humans have lost access to the ordering principles inherent in the reality of which they form a part" (p. 170). Rather than graciously accept their participation in the mysterious reality of the whole, they objectify and systemize reality, thereby distorting it in a fundamental way that keeps them from the experience of the divine ground that actually is their reality. Lacking the experience of ordering reality, they can neither resonate with symbols of reality nor themselves express such symbols.

According to Voegelin, human consciousness exists in the tension "in-between" (i.e., the *metaxy*, a Platonic term) ignorance and knowledge and "in-between" temporal and timeless reality. The experience of philosophical reason is the participatory experience that recognizes itself as a deepening involvement in the divine ground which is rational.

Thus, for Voegelin faith and reason are not in juxtaposition, as they are for Strauss. They are parts of the same experience of participation: "The human soul has reason (nous) that yearns after the perfection of itself in God" (p.

248). Strauss, in contrast, keeps the divine and human apart both ontologically and experientially (see Glen Hughes's comments on the "Strauss-Voegelin Correspondence," *Review of Politics*, 1994, 337–57). For Strauss the authority of the Torah is by faith not by reason, whereas for Voegelin the Torah is a symbolic expression of human participation in and openness to the experience of the divine ground (which for Voegelin is the experience of philosophy). In short, the experience of the theologian is not the experience of the philosopher for Strauss, while for Voegelin it is.

Despite their differences regarding the nature of reason and revelation, Strauss and Voegelin converge: Both "were worried that liberal principles would ultimately undermine liberal goals. Consequently they advocated a postliberal order—a society dedicated to basic liberal principles but fortified by a widespread belief that these principles reflect a normative order" (p. xi). How such a normative order is compatible with individualism remains the fundamental issue.

This book began as an attempt to define what it means to be a conservative in modern America. To some extent it maintains that original intention but focuses on Strauss and Voegelin: The last chapter is entitled "Strauss, Voegelin, and the Conservative Imagination." The author does not attempt to show the influence of Strauss and Voegelin on specific conservative thinkers; rather, he considers them to be vital philosophical critics of modernity and therefore sources of clarifying insight for conservatives. The main fissure within conservatism today McAllister considers to be between those who "seek to conserve the American liberal tradition by saving it from its own excesses" and those who "reject root and branch liberal ideas as products of modern, Enlightenment principles" (p. 273). The latter he labels reactionaries and aligns himself with that position: Contemporary conservatives "no longer have the luxury of conservation, but of necessity they must become reactionaries who call for a return to Christian faith and to political philosophy as a normative enterprise" (p. 278). Calling for a return to Christian faith seems to me to be inconsistent with Voegelin's welcoming of new expressions of the experience of philosophy, that is, new expressions of participation in the divine ground. In *Anamnesis*, p. 90, he writes: "Plato especially was very much aware that man, in his tension toward the ground of existence was open toward a depth of divine reality beyond the stratum that had revealed itself as the nous; as a philosopher he left consciousness open to the future of theopany, to the pneumatic revelations of the Judea-Christian type as will as to the later differentiations of mysticism and of tolerance in doctrinal matters."

Robert Devigne's book, *Recasting Conservatism* (1994), discusses Oakeshott and Strauss in relationship to British and U.S. conservatism. McAllister's work is the only one I know that considers Strauss and Voegelin together in the context of both U.S. conservatism and the history of political thought. His reading of both Strauss and Voegelin is nonpartisan, fair, and perceptive. He has a deft command of the philosophical literature, and his writing is lucid, graceful, and in places beautiful. While I think that John Ranieri's *Eric Voegelin and the Good Society* (1995) offers a deeper philosophical understanding of Voegelin alone, McAllister's presentation makes Voegelin's thought more accessible, especially to nonspecialists. A similar point applies to McAllister's discussion of Strauss: Nonspecialists will especially appreciate the clarity with which McAllister has digested Strauss's work. This is not to say that Straussians and Voegelinians will be disappointed; on the contrary, both will come away appreciative of the understanding McAllister makes available (see, e.g., the discussion of Strauss and Nietzsche, pp. 136–50, and Voegelin on consciousness, pp. 223–39). By juxtaposing these two great thinkers (which no other book-length publication has done), fundamentally defining characteristics of each emerge in sharper relief, for example, the religious expression of reality in Strauss and the mysticism of the philosophical experience for Voegelin.

The Politics of Faith and the Politics of Scepticism. By Michael Oakeshott. Edited by Timothy Fuller. New Haven and London: Yale University Press, 1996. 192p. $25.00.

Paul Franco, *Bowdoin College*

Since Michael Oakeshott's death in 1990, Yale University Press has been bringing out a number of his writings which he chose, for whatever reasons, not to publish during his lifetime. This is the third such book to appear—the first, *Religion, Politics and the Moral Life* (1993), contained both previously published and unpublished material—and in some ways it is the most interesting so far.

The typescript upon which the book is based is undated, but its themes and some of the references suggest it was written in the early 1950s. A reference to the second volume of the *Cambridge Economic History of Europe* indicates that the manuscript could not have been completed before 1952. (In his otherwise excellent introduction to the book, the editor, Timothy Fuller, dismisses this reference as mistaken, but I think it can be rendered plausible if, instead of vol. 2, chap. 7, p. 9, one reads vol. 2, chap. 7, sec. 9.) And the opposition of the politics of faith versus the politics of skepticism upon which the book is based runs through a number of Oakeshott's writings in the early 1950s, appearing for the last time in his famous essay "On Being Conservative" (1956), where he opposes the politics of skepticism to the politics of passion. After that essay—beginning with "The Masses in a Representative Democracy" (1957) and the 1958 lectures on *Morality and Politics in Modern Europe*, and continuing on through *On Human Conduct* (1975)—Oakeshott tends to characterize the poles of the modern European political consciousness in terms of the dichotomy between the morality of individuality and the morality of the antiindividual instead of the dichotomy between the politics of skepticism and the politics of faith or passion.

Despite this subtle shift in formulation, the concern that animates *The Politics of Faith and the Politics of Scepticism* remains the same as that found in the later writings: namely, to understand the character of modern European politics as it has disclosed itself since roughly the sixteenth century. And, as in his later writings, most notably the third essay of *On Human Conduct*, Oakeshott argues that this character is not something simple or monolithic but, rather, complex and equivocal. The modern European political consciousness is a divided consciousness, and the politics of faith and the politics of skepticism constitute its poles.

The politics of faith and the politics of skepticism stand for two radically opposed understandings of the activities and office of government in the modern world, according to Oakeshott. According to the first, governing is understood as the pursuit of perfection, where "perfection" denotes "a single, comprehensive condition of human circumstances" (p. 26), whether it be moral virtue, religious salvation, or material prosperity. Implied in such a perfectionist conception of politics is the idea that governing is also an unlimited or "omnicompetent" activity, unconcerned with formalities and positively disposed toward power. Power is in many ways at the heart of this conception of politics. The politics of faith views government as an immense reservoir of power to accomplish its grandiose purposes. Indeed, such a conception of politics only became possible, according to Oakeshott, with the vast increase in power in the governments of early modern Europe. In this latter regard, he interestingly argues, somewhat at odds with the view he takes in his later writings, that "the politics of faith is not the parent of a great increase in the quantity of power at the disposal of government, but a child of a circumstantial enlargement of that power" (pp. 45–6). Oakeshott sees Francis Bacon as the first and most adventurous theorist of the perfectionist politics of faith. And he explores it in two of its most influential versions: the religious and the economic.

The politics of skepticism is in all respects the opposite of the politics of faith. It, too, is the creature of the "circumstantial enlargement" of the power of European governments in the fifteenth and sixteenth century. But instead of dreaming of all that might be done with this newly acquired power, the politics of skepticism set itself to specifying more narrowly the purpose toward which such power might be used. According to this skeptical conception of politics, governing is a limited and specific activity. It is not concerned with perfection or truth but only with reducing the occasions for conflict among individuals who, pursuing a vast variety of enterprises, are apt to come into conflict with one another. Governing here is understood to be a judicial, not a managerial, activity; and the image of the ruler is that of a referee, not that of a leader. Oakeshott associates a number of thinkers with this skeptical understanding of politics—his lists are always intriguing and meant to provoke—but the names which appear most frequently in connection with it are those of Montaigne, Pascal, Hobbes, Hume, and Burke. The repeated invocation of the last thinker in this book suggests that it was probably written before 1954, when Oakeshott's attitude toward Burke began to become more ambivalent.

These, then, are the poles of the modern European political consciousness; and they have stamped our understanding of the activity of governing in the modern world as well as the political vocabulary we use to describe it—words such as "right," "security," "democracy," and so forth—with ambiguity. But Oakeshott does not write to eliminate this ambiguity. The conclusion he particularly wishes to avoid is "the fruitless conclusion that a virtuous politics would seek simplicity and 'shun ambiguous alloy'" (p. 18). And this constitutes one of the most interesting aspects of this posthumous book, serving as a corrective to what sometimes appears as a sterile purism and hostility to ambiguity in Oakeshott's later published writings. Neither the politics of faith nor the politics of skepticism, according to Oakeshott, constitutes a self-sufficient or concrete understanding of politics in the modern world. Each, taken by itself and in an uninhibited form, displays a self-destructive character.

That Oakeshott makes this argument in connection with the perfectionist politics of faith does not, of course, surprise us. But that he makes it in connection with the politics of skepticism does seem noteworthy, especially in the light of the highly procedural and formalistic outlook of his later writings. The skeptical style of politics, Oakeshott argues here, while eminently suitable to static conditions of society, is "sluggish" and unprepared when it comes to genuine emergencies; and in its moderateness, it is incapable of evoking love or gratitude from its subjects. Most important of all, the skeptical style of politics seems strangely irrelevant in the current activist climate of politics. In this climate, "the sceptical style must appear as an unintelligible piece of sophistication" (p. 109) or, worse, an expression of frivolity.

Given the self-destructiveness of both the politics of faith and the politics of skepticism taken by themselves, Oakeshott concludes that our task today should be not to opt for one or the other of these extremes but to preserve and exploit the complexity and ambiguity of our political tradition. To this end, he resuscitates Halifax's *Character of a Trimmer* as the character most appropriate to our situation. The "trimmer" is one who does not put all the weight of a ship on one side or point the sail in a single direction but who constantly makes adjustments in response to particular circumstances in order to keep the boat on an even keel. In terms of the dichotomy which defines our political tradition, the trimmer will be concerned to prevent our politics from running to either of the extremes of faith or skepticism and to cultivate the middle region between them. This does not mean, however, that the trimmer will always be simply in the middle. In our current circumstances, for example, which Oakeshott sees as dominated by the politics of faith, the trimmer will throw his weight to the other side of the boat and seek to renew the skeptical pole of our politics.

One may, of course, question whether this latter reading of our political predicament is entirely applicable to our situation today. In a world in which all parties agree that the era of big government is over, it is not obvious that the perfectionist politics of faith is so clearly dominant any longer. In this respect, Oakeshott's analysis seems a little dated, more attuned to the ideological climate of the pre- and postwar period than to our more muddled and confused situation at the end of the twentieth century. While this does not detract from his overall counsel of moderation and the eschewing of ideological simplicity, it does make it somewhat less determinate as a guide for what we should do today. But to seek such practical guidance is perhaps too much to ask of a book that is now more than forty years old as well as too much to ask of a thinker who never claimed to give "mistake-proof answers" to political questions. *The Politics of Faith and the Politics of Scepticism* is concerned to trace the deepest and most permanent features of the modern European political landscape over the last five hundred years, and this it does in an original, insightful, and frequently eloquent manner. We are fortunate that the book has finally seen the light of day.

Current Issues in Public Choice. Edited by Jose Casas Pardo and Friedrich Schneider. Cheltenham: Edward Elgar, 1996. 267p. $79.95 cloth, $29.95 paper.

Hugh Ward, *University of Essex*

Rational choice theorists have shown that institutions matter because they structure equilibria. This edited volume of papers given at the 1994 Annual Meeting of the European Public Choice Society should reflect the fact that European institutions are different from those in the United States. Yet, there are authors whose arguments are supposed to be general ones who fail to recognize this. In Brennan's model, voters choose between packages consisting of tax levels together with redistributive grants. He shows that: The median voter's preferred package is the equilibrium; there is no redistribution or overall effect on public expenditure; and the outcome is inefficient. This model is inappropriate for systems in continental Europe, where coalition governments are the norm. Even if the tax/redistribution issue is bracketed off from others, with coalition governments there is no good reason to suppose that outcomes will converge on the median. Redistribution is often the result of deals made by coalition partners to patch up ethnic, linguistic, and class cleavages. This is socially functional and is likely to be ethically acceptable to anyone who does not adopt the extraordinarily narrow normative perspective of many public choice theorists.

Peter Lewisch considers the use of economic man assumptions in the design of legal institutions. Among other interesting questions, he asks whether legal institutions should be designed to encourage rational behavior. Gordon Tullock argues that the enforcement of contract law in Anglo-Saxon legal systems has strong analogies with rent-seeking. The legal systems of continental Europe may avoid some of the problems. Both of these authors clearly do recognize that institutions matter, but the message still has not sunk in enough in public choice, which often models every system as if it is the United States.

Friedrich Schneider discusses Swiss evidence which suggests to him that the use of referenda reduces upward pressure on taxation and public spending. He argues that representative democracy weakens the link between outcomes and the median voter's preferences and encourages rent-seeking. So the dangers of a federal Europe creating incentives for rent-seeking and upward pressures on taxation and public expenditure can be reduced by Swiss-style referenda. It is difficult to see how meaningful participation rates could be obtained without the degree of decentralization of the Swiss system; yet, this would preclude the harmonization of taxes across the European Union that is crucial for the single market.

Jon Elster argues that constitutions are self-binding devices—hence the inbuilt restrictions on their modification. While self-interest is important in constitution building, so are reason and passion. So framers make constitutions to prevent damaging expressions of self-interest or conflicts based on identity, even though their own short-term interests are harmed. Elster shows that public choice need not paint a one-dimensional picture of human motivation and rationality, a theme also found in several other contributions.

Bruno Frey and Iris Bonet discuss experimental evidence which suggests that the degree of communication between individuals matters in public goods and redistribution games, because it activates norms. So communication may lead to redistribution or public good provision without the need for state intervention. Ulrich Blume and Leonard Dudley argue that different phases of economic growth in Europe since the tenth century can be explained culturally. Individuals have a genetic predisposition to solve collective problems through sharing, obeying others in a hierarchy, or communicating information. Group leaders appeal for members on the basis of these sources of identity. Shifts in technologies of control of group members may lead to the domination of groups centered on one form of identity. This effects the economy, because it is associated with greater dead-weight losses due to the costs of controlling group members. Although spatial competition between group leaders for members with preferences over identities is gestured at, no attempt is made to find equilibria.

Blume and Leonard assume that static inefficiencies translate into poorer dynamic economic performance, a public choice shibboleth for which there is no adequate theoretical or empirical foundation. Orthodox positions are taken in other papers, too. Chares Rowley and William Thorbecke show just how empirically sophisticated modern public choice theory can be in tracing out complex forms of rent-seeking that run through congressional oversight committees to apparently independent bodies like the U.S. International Trade Commission. They obviously regard this as undesirable and fail to ask whether restrictions on free trade are normatively justifiable on grounds such as the protection of rights. A standard public choice reply might be that individual rights are incommensurable with economic values, such as losses due to restrictions on free trade. In contrast, Sen's chapter argues that individual rights are commensurable with other consequences of social action, allowing meaningful social choices to be made, even if a complete social ordering cannot be derived.

As Francesco Forte says in his chapter, public choice theory can make use of game theory. For instance, James Buchanan uses the prisoner's dilemma to illustrate his argument that majority rule coerces minorities. He goes on to argue that limiting the possible outcomes to those in which everyone uses the same strategy, although it can lead to inefficient outcomes, nevertheless eliminates the coercion of minorities. Yet, individuals are still made to do what they would not do otherwise, even in Buchanan's constrained majority rule games. This leaves the reader unclear about what coercion means. Buchanan also suggests that public choice needs to consider the way in which outcome sets are produced by prior constitutional limitations. This is of little substantive significance when set against the structural and ideological processes limiting feasible outcomes in capitalist societies. For example, as Vani Booroah says in his chapter, public choice theorists should take seriously Kalecki's argument that full employment is off the agenda given the structural power of business.

As the title of this volume suggests, the editors aimed to bring us the latest work in public choice. Actually, several contributions are far from original. For instance, Ulrich Witt shows that in mixed-motive games which players learn from others' success, cooperative patterns of play can sometimes be immune to invasion by noncooperative patterns, so long as cooperative players are willing to take revenge on defectors. These results will not surprise readers of Axelrod's work, which is not cited. The variable quality of the chapters makes this a volume to dip into selectively.

Democracy's Discontent: America in Search of a Public Philosophy. By Michael J. Sandel. Cambridge, MA: Harvard University Press, 1996. 417p. $24.95.

Susan Okin, *Stanford University*

Historians of the United States have traced eclipses of community by individualism as far back as the 1650s. In *Democracy's Discontent*, Michael Sandel presents the last half-century as a period of such civic breakdown, rewriting U.S. history as a morality play. The forces of good are republicans, by whom "liberty is understood as a consequence of self-government" (p. 26), who are focused on the common good, who think government should be concerned with the characters and civic virtues of its citizens, and who espouse an "encumbered" concept of the self—as "obligated to fulfill ends we have not chosen" (p. 12). The negative forces are liberals, who believe that persons are "free and independent selves, unencumbered by moral or civic ties they have not chosen" (p. 6), with rights that should "trump" majority decisions, and who think government should avoid questions of morality and the good life and not concern itself with its citizens' character. The good players include Jefferson, Lincoln, Brandeis (most of the time), F.D. Roosevelt before Keynesianism corrupted him, Martin Luther King, and Robert Kennedy (in his last few years). The bad players include the majority of Supreme Court justices from 1943 on, Lyndon Johnson, John Rawls, and Ronald Dworkin, all of whom reflect the baneful influence of Kant or J.S. Mill. A deep and primary source of the discontent that afflicts the citizens of the United States is "the theory implicit in our public life" (pp. x and 278), the triumph of liberalism over the republicanism that shaped our society.

Sandel's Manichean worldview requires distorting the views of virtually all he discusses. In fact, all the main actors on both sides of his moral war were liberals *and* republicans, by most of his criteria, and none were republicans by one of them. All thought that freedom depends on both individual rights and representative government; that governments inevitably legislate some moral matters, affecting the character of their citizens, but should avoid many controversial religious and moral positions; and that political and legal decisions should take some commitments and characteristics of persons into account, but not others. Yet, none thought rights were important only insofar as they foster civic virtue.

In this book, however, liberals are not concerned with government's effects on citizens' values, thinking instead that government should be neutral and merely protect individual liberty, which is "defined in opposition to democracy" (pp. 25, 28). Thus, Sandel's Mill apparently never wrote "On Representative Government," in which the real Mill clearly states that the *most* important criterion of good government is the intellectual and moral improvement of its citizens and explains how self-government contributes to this end. As for Kant, he does indeed state the "memorable" view Sandel quotes: that "the problem of setting up a state can be solved even by a nation of devils" (p. 322). Kant means, however, as he says, "setting up," and he goes right on to say (whereas Sandel does not) that while "we cannot expect . . . moral attitudes to produce a good political constitution; on the contrary, it is only through the latter that the people can be expected to attain a good level of moral culture" (*Kant's Political Writings*, ed. Reiss, 1970, pp. 112–3). Likewise, you would never know from Sandel's account of Rawls's liberalism that Rawls explains at considerable length in both of his books how a well-ordered society must maintain a sense of justice and other political virtues in its citizens.

Republicans, according to Sandel, begin "by asking how citizens can be capable of self-government" (p. 27) and are concerned with individual rights only insofar as these promote the common good or foster the civic virtues needed for self-government. Thus, Sandel's Jefferson seems not to have written the Declaration of Independence, which of course begins with the "self-evident" truths that the Creator gave to men "unalienable Rights" to life, liberty, and the pursuit of happiness and *then* states that governments are instituted to "secure these rights." Neither does Sandel's Brandeis much concern himself with individual freedoms except as aids to self-government, though the real Brandeis not only defended "the right to be let alone," as Sandel notes (p. 95), but also named it "the most comprehensive of rights and the right most valued by civilized men (*Olmstead* versus *United States*, 277 U.S. 438, 478 [1928])." Brandeis, like Jefferson, while a strong believer in republican self-government, clearly thought of it largely as the best means of preserving the freedom essential to individual happiness, rather than thinking of "freedom [as] essentially connected to the good of self-government"—a meaning Sandel imputes to the passage he quotes by diverting its first, key clause into a footnote (p. 79 and n. 116).

Even helped by such scholarly techniques, Sandel's account of the twentieth century's liberal eclipse of republicanism is unpersuasive. The timing of the supposed eclipse keeps changing: Sometimes it ranges from the late 1930s until the 1960s (though the public philosophy that allegedly inspired it was not articulated until the 1970s); sometimes it is attributed to "the last fifty years"; sometimes it is traced to the Lochner era of constitutional law, beginning in 1905. But whichever the period Sandel's evidence fits poorly. Why do we find the civil rights movement, which Sandel (eccentrically) labels "republican," arising in the 1950s and 1960s, a period of supposed liberal ascendancy? Why was the civic-spirited republican Kennedy so popular with voters in 1968? Why did almost all the programs that Sandel suggests may reinspire the civic virtues originate in the 1940s, 1950s, 1960s and 1970s—the decades of liberal civic impoverishment? As for the supposed influence of political theory over practice, *does* it explain why between 1940 and 1943 the Supreme Court's attitude about compulsory flag salutes by school children suddenly shifted from republican to liberal? Several passages of the Court's opinion in the latter case—with which, Sandel announces, "the procedural republic had arrived" (p. 54)—indicate that it was the shadow of Hitler, rather than that of the teenaged John Rawls or the infant Dworkin (or even the words of the long-dead Mill) that hung over the Court's abrupt change of heart (*West Virginia State Board of Education* versus *Barnette*, 319 U.S. 624, 627 and 641).

In Sandel's account of the recent history of freedom of religion and speech, privacy rights, and family law, courts and legislatures have moved ever farther toward conceiving of persons as freely choosing, unencumbered selves. While earlier defenses of religious freedom (such as Jefferson's and Madison's) "had nothing to do with a right to choose one's beliefs" (p. 65), recent defenses are infused with notions of choice, which Sandel finds offensive to the true religious believer. Most of the Supreme Court's errors arise from its refusal to take seriously people's "encumbrances," such as deeply rooted moral or religious beliefs.

The problem with the diagnosis emerges as soon as Sandel presents his cure. For in suggesting how the Court should have deliberated, he takes account only of those "encumbered selves" with whom he is most sympathetic. Thus, in the *Skokie* free speech case, we hear of encumbered Holocaust survivors but not encumbered civil libertarians; in *Hardwick*, the case of homosexual privacy, Sandel attends to the encumbered homosexual (so long as he is monogamous) but ignores the encumbered homophobic religious believer; in advocating new divorce laws, he favors the encumbered traditional wife over the encumbered feminist or role-sharing husband. Since the reason these cases are difficult is that they involve persons or groups with *conflicting* deeply held beliefs, no solution can result from focusing on the encumberedness only of those on one side.

Liberalism's worst fault, as Sandel presents it, is its failure to engage the serious moral issues of political life. Thus, we might expect Sandel the republican to engage them, but he does not. The closest he comes is to "gesture towards" some positions and away from others. On abortion, for example, he never comes out firmly against choice, but he likens leaving this moral decision to women to leaving the similarly serious moral issue of slavery to the states to decide. He does not actually recommend denying subsistence to children because of their parents' sexual behavior but, again, his gestures come perilously close. He chastises the Supreme Court both for upholding and for restricting religious freedom, in similar circumstances. While the Court was apparently wrong not to force the Jehovah's Witness child to salute the flag (he asks: "If not by compulsory flag salutes, how may the state cultivate a common citizenship?" [p. 54]), the Court's refusal to permit an Orthodox Jew to wear his yarmulke in the military was also wrong, since it failed to take account of the Jew's encumbered self. But what happened to common citizenship here? If not in its armed forces, where can the virtuous republic foster it?

What civic virtues matter here and now? Given that those lauded by Sandel's republican heroes include conflicting ones (such as deference to authority and independence of thought and character), we expect, but do not get, guidance as to which he thinks we should foster and why. Neither, though he worries about the present scale of economic power, does he engage the tough question of whether the independence required of republican citizens can coexist with capitalism on any scale at all. Instead, he states: "The civic virtue distinctive to our time is the capacity to negotiate our way among the sometimes overlapping, sometimes conflicting obligations that claim us"—not an easy task, for "it is easier to live with the plurality between persons than within them" (p. 350). This view, however, depends on a curious, morally solipsistic refusal to recognize that we are not only persons each with his or her *own* potentially conflicting loyalties. We also live in a country and an era in which a plurality of conceptions of the good life among persons is a fundamental fact—one with which liberal conceptions of justice and rights have had far more success in dealing than have republican conceptions of encumbered selves.

If Sandel argued throughout that there have always been strong liberal and republican currents in U.S. political discourse (as he once briefly and partially acknowledges [p. 6]), and that these currents often coexist in the beliefs of the same thinker, his book would be far more plausible. If he tackled and declared himself on the difficult moral issues, it would be more engaging and far more courageous. As it is, *Democracy's Discontent* provides neither a reliable history of political thought in the United States nor a coherent argument about civic virtue.

Identities, Politics, and Rights. Edited by Austin Sarat and Thomas R. Kearns. Ann Arbor: University of Michigan Press, 1995. 439p. $65.00.

Legal Rights: Historical and Philosophical Perspectives. Edited by Austin Sarat and Thomas R. Kearns. Ann Arbor: University of Michigan Press, 1996. 315p. $44.50.

Nicholas Xenos, *University of Massachusetts at Amherst*

Rights are funny things. Generally thought to be grounded somewhere—historically, rationally, or naturally—rights are the promiscuously spent common coin of U.S. political discourse. Each generation, it seems, finds new rights to assert or old ones to resurrect; every claimant to privilege or redress against privilege brandishes a right. The scene is almost as cacophonous in the academic marketplace, where the analysis, derogation, and/or perpetuation of "rights talk" constitutes a vibrant commerce. Just how widely the currency of rights has been strewn across various disciplines and theoretical schools can be gleaned from these two collections edited by Austin Sarat and Thomas R. Kearns, each of which constitutes a separate volume in the Amherst Series in Law, Jurisprudence, and Social Thought but which together contain nineteen papers originally presented at an Amherst College conference, "The Paradoxes of Rights," in 1992. Among the authors present here are the legal scholars Martha Minow, Steven Shiffrin, Morton J. Horwitz, and Pierre Schlag, political theorist Jeremy Waldron, the literary scholar Annabel Patterson, and historian Thomas Haskell.

Identities, Politics, and Rights comprises eleven of these papers. The editors claim that the collection "challenges liberal theory and suggests productive ways to imagine postliberal understandings of rights" (p. vii). This is a large claim, indeed, not least for the proposal that rights can (and will) be understood in distinction from liberal political institutions and theory. The fact that rights are not independent of politics is one of the elements of this claim, but hardly the most innovative or unexpected. More central is the notion that rights are constitutive of a specific notion of the subject, and it is primarily among the essays grouped in the first half of this volume, under the heading "Rights and the Constitution of the Self," that this important claim is taken up and argued. Wendy Brown develops this idea through a reading of Marx's essay "On the Jewish Question," in which she brings Marx into conjunction with Foucault. She argues that this perspective reveals "how rights pervasively configure a political culture . . . and discursively produce the political subject," while also permitting us "to grasp the way in which disciplinary productions of identity may become the site of rights struggles that naturalize and thus entrench the powers of which their identities are the effects" (p. 114). Brown goes on to offer criticisms of the work of Patricia Williams and Catherine McKinnon: the former for failing to see the ways in which rights can confer a demeaning sense of personal responsibility for the "failures" of poverty and the latter for "installing identity in the law" (p. 129) through her effort to encode the experiences of "woman" in legal discourse. Brown suggests that while the empty category of abstract rights may be helpful in formulating challenges to the status quo, the filling of this category with substantive content has the paradoxical effect of stabilizing the status quo by giving the appearance of universality to historically constituted categories of personhood and/or identity.

Brown's paper certainly represents a challenge to liberal theory, but one that gives little consolation to the possibility of postliberal rights. The same may be said, less predictably perhaps, for the paper by anthropologist John Comaroff, "The Discourse of Rights in Colonial South Africa: Subjectivity, Sovereignty, Modernity," in which the constitutive and naturalizing character of rights is brilliantly described and argued. Comaroff shows how complex was the process by which the indigenous peoples of South Africa became—usually, but not always, to their detriment—rights-bearing individuals. Central to his story is the role of evangelists in the colonial mission, with their attention to the minute details of the everyday and their overriding aim of (re)constructing the African according to a particular image of responsible personhood, complete with the proper respect for a self-centered morality and private property. At the same time, it was the evangelicals who introduced the notion of "primal sovereignty" where no notion of separate human groupings had previously existed (p. 214). But the colonialists, in inventing names for various "tribes" also invented the things that were named: "They contrived for them an entire ethnology, dividing them into 'tribes' and ascribing to them a primordial identity based on common ancestors and origins, language and lore, culture and customs, sentiments and interests" (pp. 214–5).

Once adopted by the evangelicals, these identities were drubbed into the colonized, until the latter wound up adopting them for themselves, with paradoxical effect. On the one hand, this primal sovereignty placed the colonized within a world of identities shared by the Europeans; on the other, it established a relationship of permanent inequality between them. The primal groupings were said to have the rights associated with their primal sovereignty, but only in order to have them denied by the colonialists because their bearers were insufficiently "civilized" to claim them. The "right" of the "Bechuana" to their land, for example, was recognized in the act of European expropriation in order to protect it, "if only so that it might be 'voluntarily' alienated to European control" (p. 225). Or the right of sovereign autonomy might be recognized in the alienation of that authority to a colonial power through some sign of consent, such as a formal "petition" requesting European control on the part of the newly constituted local rulers. One legacy of colonialism is, therefore, the "double-consciousness" of rights as source of self-identity and possible resistance as well as rights as instrument of rule. Comaroff's careful analysis suggests that the discourse of rights is inherently double-sided, making it impossible to separate the possibly liberatory aspect from the role rights play in stabilizing regimes of unequal power.

Other papers in this volume share the theoretical position taken by Brown and supported by Comaroff's explication of the paradoxes of rights in the colonial setting, at least in broad outline. Political theorist Kirsty McClure, for example, like Brown, relies on a perspective informed by Foucault in her "Taking Liberties in Foucault's Triangle: Sovereignty, Discipline, Governmentality, and the Subject of Rights." This subject, she argues, is situated in the interstices of the overlapping languages of "the philosophical discourse of modernity" (p. 165) and of sovereignty, which means that it is neither prior to the law that articulates rights nor wholly consequent upon it. To illustrate her argument, McClure ingeniously works through the writings of Thomas Malthus, Robert Owen, and William Hazlitt, sorting out the different senses of rights each author acknowledged in his treatment of the situation of the poor in industrial England. She is thus able to show how conceptions of negative (defensive), positive (participatory), and entitlement rights were variously deployed or ignored not only among these writers but also sometimes within the writings of each of them as the concrete situation to which the author was responding warranted.

Though Malthus championed a view of negative rights, for example, McClure argues that this view worked to advance the cause of "governmentality even as it opposed the disciplinary apparatus of the Poor Laws as tyranny." Owen, in contrast, combined a respect for entitlement rights with support of that same disciplinary apparatus. Hazlitt, a writer usually overlooked by political theorists, emerges as the least systematic of the three—he was, after all, an essayist—but also as the most interesting of them, as he debunked both Malthus and Owen, substituting instead an inchoate image of the popular judgment and a plastic sense of rights grounded in the appetites and passions of the people (as Hazlitt called his audience) rather than in rationalist systems of thought. McClure is drawn to Hazlitt's messy sense of popular judgment because it invokes an "unruly subject" (p. 186) that serves to highlight, by contrast, the ruled subject at the core of Foucault's theory of modernity while at the same time indicating the excessively monologic character of that theory. What McClure claims is shown by the confluence of Malthus, Owen, and Hazlitt is the always contested notion of rights and, in Hazlitt's case, the always present possibility of a fugitive subjectivity at least in part uncontrolled by the orders of sovereignty and discipline. But the essential point is that that possibility is contained within the entire fabric of rights discourses, and thus one might conclude that there can be no currently available conceptualization of rights outside the liberal understanding.

Among the authors who are primarily concerned with the question of the self, it is Drucilla Cornell, perhaps surprisingly, who offers what amounts to a dissenting opinion on the limits of liberal rights in her essay, "Bodily Integrity and the Right to Abortion." Cornell's argument is centered on the claim, contained in her opening sentence, "that the right to abortion . . . is absolutely essential to the establishment of the minimum conditions for individuation" (p. 21). This claim, in turn, rests on the "fundamentally liberal idea" (p. 26) that individuation is tied to the subject's control over the narrative within which the decision concerning an abortion is situated. By relegating control over that narrative to the state, the womb is separated from the notion of a woman's self, thus denying a sense of bodily integrity, and so her "coherence" (p. 27). But more, Cornell also takes the stand that the question of the balance between "frustrating a mere biological investment in human life"—that is, the fetus—and "a human investment"—that is, the woman's selfhood—is ultimately to be decided by the woman (p. 72). Another solid liberal position. So, despite the reliance upon Lacanian theory and the constant emphasis upon narrative—Cornell goes so far as to confuse the first-person narrative of a character in a novel with the testimony of a "working class voice"—this postmodern understanding of rights turns out to be not in the least a postliberal one, as she ironically admits in the observation that "perhaps in the end I am more liberal than [Ronald] Dworkin" (p. 74). Perhaps.

The rest of the essays in this volume and those collected in *Legal Rights* are more diffuse in orientation, and no comparably strong, connected argument or theme emerges from them. Disciplinary boundaries are generally obeyed, so the philosophical and historical perspectives promised in the latter are delivered, but with the "and" of the subtitle doing all the work. None of the contributors succeeds in presenting even the barest outline of what a postliberal understanding of rights might be that would challenge the theoretical implications of the Brown, Comaroff, and McClure essays, and the evidence suggests that there is good reason for this lacuna. If we are to gain purchase on postliberalism, it appears, we must do so in a currency foreign to the discourse of rights.

Only Paradoxes to Offer: French Feminists and the Rights of Man. By Joan Wallach Scott. Cambridge, MA: Harvard University Press, 1996. 229p. $27.95.

Joan B. Landes, *Pennsylvania State University*

No more dramatic case exists of women's paradoxical relationship to modern concepts of citizenship than that of France, which first established universal (male) citizenship and abolished racial barriers in the early years of the French Revolution. In contrast, women only received the right to vote at the end of World War II. Yet, calls for women's suffrage surfaced, and were repulsed, in the revolutions of 1789 and 1848 as well as during the late nineteenth and early twentieth century. Against the failures of French republicanism, Joan Wallach Scott examines key moments in the history of French feminism. She compares appeals to equality and arguments for sexual difference in the writings of four prominent feminists: the eighteenth-century playwright and author of the "Declaration of the Rights of Woman," Olympe de Gouges; the utopian socialist feminist Jeanne Deroin; and third republic feminists Hubertine Auclert and Madeleine Pelletier, the latter a radical individualist, the former a proponent of a more collective, social vision. Rather than advocating for one or another position, Scott insists that "competing feminist visions have taken one side or another of the opposition between the abstract individual (which counts women the same as men) and the individual self-defined through sexual difference (which insists on their radical unlikeness)" (p. 173).

It has been said that the historian must straddle the line between making the past familiar and never forgetting that it is a strange territory. Readers of this book will enjoy discovering (or rediscovering) four compelling women, while marveling at how the terms of earlier feminism are at once familiar and strange. Rather than taking the category of women for granted as the subject of feminist discourse and politics, Scott argues that feminist agency is itself profoundly paradoxical: "To the extent that it acted for 'women,' feminism produced the 'sexual difference' it sought to eliminate. This paradox—the need both to accept *and* to refuse 'sexual difference'—was the constitutive condition of feminism as a political movement throughout its long history" (pp. 3–4).

Only Paradoxes to Offer, a phrase Scott borrows from Olympe de Gouges, offers a model for theoretically informed gender history, urges an historical approach to feminist theory, and continues to plead for the usefulness of poststructuralist theory inside the discipline of history (*Gender and the Politics of History*, 1988). Thus, Scott rejects teleological history and faults what she terms the "typical approach to the history of feminism" derived from nineteenth-century feminists for its naïvely fashioned story of cumulative progress (p. 1). In addition, she criticizes biographical approaches for fortifying "the notion that agency is an expression of autonomous individual will, rather than the effect of a historically defined process which forms subjects" (pp. 15–6). Despite the focus on four individuals, this is a book about the discursive practices—epistemologies, institutions, and actions—that produce feminist political subjectivity or agency "even when it is forbidden or denied" (p. 16). Without attempting a comprehensive survey of French feminism, this book contributes a probing intellectual history of the central questions in modern feminist thought which will also add much to contemporary feminist inquiry.

Scott foregrounds Olympe de Gouges's reliance on the concept of imagination in relation to Enlightenment debates over the possibilities and dangers posed by the imagination to rational social order; as well as imagination's role in shaping new meanings of masculinity and femininity in the eighteenth century. She links Gouges's demands for sexual equality and female citizenship to her impassioned appeals to the active imagination, and she shows how her posture offered ammunition to Jacobin male critics of women's active citizenship. As Gouges emerges as more than a simple proponent of rationalist individualism, so Jean Deroin is not merely a "difference" feminist. Scott suggests that in arguing for complementarity of the sexes, claiming politics as the domain of women and equating the household and the state, Deroin went beyond claims for simple parity. Scott links Deroin's outlook to Second Republic notions of rights and duties and arguments for the right to work. Deroin's efforts to expose the "lie" of a republic that refused political rights to women were continued by Third Republic feminist Hubertine Auclert. Within the context of competing republican and socialist perspectives on the social division of labor, and its relationship to politics, Scott charts Auclert's defense of women's rights as part of a broader appeal to democratic or popular sovereignty. Finally, she examines Madeleine Pelletier's turn away from the discourse of "the social," but not socialism, toward a radical individualist perspective. Scott addresses Pelletier's ambivalence to sexuality and her strong defense of birth control, her defense of mass democracy, and her worries about massified individuals.

As Scott freely admits, her own thinking has taken shape within a particular discursive context and will be open to revision. She offers a persuasive account of the discursive underpinnings of gender exclusion in a central ambivalence within the philosophy of individualism: "The universal individual who exercised the political rights of 'man' was at once abstract and concrete; difference from a woman (whether a matter of desire or reproductive function) secured both his typicality *and* the boundaries of his individuality" (p. 10). Whereas abstraction serves as the basis for inclusion (equality and universality), it can be and was used as a standard for exclusion insofar as some persons were defined as nonindividuals, lacking the necessary traits by which one establishes individuality in the first place. Gender was one of several criteria by which whole categories of persons were established as different, inferior, and lacking in the rights accorded to the free individual. Like Simone de Beauvoir, Scott rightly observes that sexual difference has operated in a hierarchical and nonreversible manner. Whereas the female is "the sex," "the other" to the abstract but masculine individual, he is never the woman's other. Nor do women escape from these dilemmas: By calling attention to their own contradictory exclusions from universalist ideology, women displayed their own "self-contradictoriness," the "disturbing spectacle presented by paradox" (p. 11). Stated otherwise, by acting in public women risked making a "spectacle" of themselves. As with the dilemmas posed by equality feminism, Scott exposes the self-contradictory positing of gender difference as a way of eliminating its very basis. Her aim is not to resolve but to demonstrate the contradictory alternatives imposed by modern gendered political subjectivity.

Plato Rediscovered: Human Value and Social Order. By T. K. Seung. Lanham, MD: Rowman and Littlefield Publishers, 1996. 394p. $64.00 cloth, $26.95 paper.

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This is not a work of conventional Classical scholarship. T. K. Seung decries Plato scholars for not taking pride in the "power and truth of Plato's philosophy" (p. 302). Their "pedantic . . . logical analysis and textual exegesis" have succeeded only in consigning Plato's works to "the mausoleum of obsolete classical texts" (pp. 301–2). Throughout the book, Seung attempts dramatically new accounts of central aspects of Plato's thought. In the final chapter, "Platonic Legacies," he defends the truth of Plato's teaching.

Though Seung describes his main concern in explicating Plato as political philosophy (p. ix), he devotes little attention to Plato's moral and political views per se, as opposed to their ontological underpinnings. He discusses twenty-two separate dialogues of "the Platonic Archipelago" (p. xiv), working out a consistent account of the development of central themes in Plato's metaphysical and epistemological views and their implications for political theory.

In important respects, Seung's account is familiar. He traces the movement from the early Socratic dialogues to the great works of Plato's middle period—the *Meno*, *Phaedo*, and *Symposium*—which introduce the theory of forms, which undergirds Plato's political theory in the *Republic*. At this point, Plato enters a self-critical phase, renouncing the excesses of his previous theory of forms, especially in the *Parmenides*, and replacing it with a trimmed down account, to which Seung refers as the "bedrock" version, in comparison to the "skyscraper" version in the middle works. The bedrock version retreats from the metaphysical extravagance of self-predicating, exemplar forms. Forsaking the erotic mysticism of the middle works, Plato pays more attention to the phenomenal world. The new political theory of the *Laws* is deeply rooted in Greek history and political institutions, in contrast to the metaphysically grounded ideal city of the *Republic*.

Although Plato scholars are a notoriously contentious lot, many would find little with which to disagree in the foregoing account. But on this frame Seung hangs an original interpretation of the second stage of the evolution of Plato's thought—the "hidden half" of Plato's philosophy (p. 291).

While the movement from the early Socratic dialogues to the middle works has been exhaustively studied and the connections among moral, psychological, and metaphysical themes well marked, works such as the *Theaetetus*, *Sophist*, *Philebus*, and *Parmenides* are furiously difficult. Scholars who have addressed them have had relatively little interest in Plato's political theory, while students of the latter have done relatively little work on this forbidding material. Seung, who is a professor of philosophy as well as government (and law), tackles the daunting task of synthesis. Briefly, he regards the traditional opinion that several of Plato's works, including late works, are *aporetic* or inconclusive as "gigantic misreadings" (p. 205). He extracts from the *Theaetetus* (normally viewed as aporetic) new epistemological views, which are combined with new semantic views drawn from the forbidding dialectical exercise in the *Parmenides* and new ontological views from the *Sophist*. Pythagorean themes from the *Philebus* and *Timaeus* are incorporated into this new theory, at the heart of which Seung locates Platonic "intuition" and "construction." According to Seung, "Platonic construction is the construction of proper measures" (p. 231). The science of proper measures is discussed in the *Statesman* and provides the underpinning for the mathematical elements of the ideal city presented in the *Laws* (esp. pp. 229–31).

One must be impressed by Seung's accomplishment in this dazzling synthesis—although I will defer on the question of exactly how plausible specific components are to specialists in Plato's metaphysics. It is clear, however, that Seung's case is weakened by his high-handed approach. He clearly forces portions of his discussion, proceeding on an abstract level, avoiding the messy details of Plato's texts—and how these have been interpreted by previous scholars—dipping into aspects of the dialogues that support his view but never systematically dealing with those that do not. Aspects of his presentation are cavalier, such as discussion of what appear to be "skyscraper" forms in the *Timaeus*, which go against his theory (pp. 237–8), the account of forms in the *Laws* (pp. 231–2), or the nature of the city-soul analogy in the *Republic* (pp. 251–2).

Throughout the book, Seung presents a plethora of original interpretations, whether relevant to his overall argument or not. Some ideas are highly suggestive, for example, that the idyllic natural setting of the *Phaedrus* signifies Plato's coming to terms with the natural world (p. 131). Other interpretations work less well, such as frequent comparisons between Socrates and Odysseus, Seung's explanation of why the discussion in the *Timaeus* is a continuation of that in the *Republic* (pp. 271–2), or the far-fetched claim that Theaetetus is a mask for Plato himself, an hypothesis that Seung pursues at length (pp. 275–82).

It is not easy to identify the audience for whom Seung's discussion of Plato is intended. The most interesting portions of the book are too difficult for most students of Plato, while specialists in the ontology of the late dialogues will be frustrated by Seung's refusal to argue out the details of his interpretation. Accordingly, in spite of its often glittering intellectuality, *Plato Rediscovered* would have benefitted from a narrower, sharper focus and greater attention to defending its provocative account of the development of Plato's thought.

Seung's main argument for the validity of Plato's normative position is, briefly, that it is the only alternative to "relativism and nihilism" (p. 294). Seung runs through a range of other possible alternatives in a few pages and finds them all wanting, as all collapse into either relativism or Platonism. There are two significant problems with his discussion, which I will mention briefly. First, the dichotomy, Platonism or nihilism, is not a convincing argument. Seung is far too dismissive of the alternatives, including the mainstream of contemporary moral philosophy, which is brushed aside. Perhaps he would respond that defensible alternative views, such as those of Rawls and his followers, are actually forms of Platonism in disguise. This raises the second point. The "Platonism" Seung affirms in the final chapter is extremely modest. He identifies any "belief in the existence of standards and principles that transcend all positive normative systems" as Platonism (p. 293). Exactly what constitutes an objectively grounded normative standard remains obscure—as does the nature of the intuition through which such standards are apprehended. But however we construe these matters, it is unclear what this minimal Platonism has to do with the substantive philosophy expressed in Plato's dialogues, toward the explication of which Seung devotes so much effort. Despite his affirmation of the truth of Plato's principles, the relation these bear to the complex philosophical system expressed in the Platonic archipelago is never explained.

The Embodiment of Reason: Kant on Spirit, Generation, and Community. By Susan Meld Shell. Chicago: The University of Chicago Press, 1996. 483p. $65.00 cloth, $21.95 paper.

Steven M. DeLue, *Miami University*

Susan Shell explains what she views as Kant's "novel definition of the world," and she demonstrates how his life's work is an "elucidation" of this definition, informing Kant's entire philosophy (p. 2). Shell provides very useful discussions of Kant's views of Leibniz, nature, Swedenborg, history, gender roles, and Rousseau. Her spectacular chapter on Kant's hypochondria is a significant contribution to Kant studies.

Shell's description of Kant's definition of the world begins with a very good discussion of Kant's understanding of humankind's relationship to nature. This relationship is important because it demonstrates a conflict, central to Kant's world view, between the inner world of human consciousness and the outer world of nature. Nature is defined in terms of activities of "unceasing creation," to use Kant's words (p. 58), in which new forms of life replace older forms that have been destroyed by nature's devastating moments of force. Everything that exists in nature must die, but death does not stop nature from advancing because nature is always at work, in Kant's words, "repairing . . . the loss with profit" (p. 57).

It is natural to ask what significance human life has in this vortex marked by the inevitability of the death of all things which come into existence. After all, humankind, to quote Kant, seeks "community . . . with the immortal being" (p. 59), and this fact suggests the need to find a basis for establishing human life's significance amid what appears as a heartless nature. Humankind satisfies this need through its unique capacity to define its own purposes in life. With this freedom, humankind creates culture and thereby gives birth to beauty and morality. Each of these dimensions manifests humankind's intention to make use of nature to support truly human ends. Through these efforts, humankind avoids, as Shell says, "a permanent rupture" with nature and instead recognizes that, as the "final end" of nature, human beings are both "nature's child and nature's master" (pp. 258–9).

The finest achievement of culture is a form of community in which individuals uphold the moral law and treat one another as an "absolute end" or, in other terms, not "as a means only" (p. 149). For Kant, comprehending what makes this type of community possible is a key concern. Our freedom to create a community in which people are treated as ends evolves only when we possess an intelligible understanding of experience, an understanding that describes the possibilities and limits presented by experience. This type of understanding presumes that whereas we cannot know directly the things that exist in the world, we can know them indirectly through our perceptions. The rational intelligence provides the concepts, such as time and space and cause and effect, that organize our perceptions, so we can have an intelligible understanding of experience. Without these concepts, our understanding of the world would be reduced to random and unintelligible perceptions. A major theme of this book, then, is to show how, in Kant, the rational intelligence creates an understanding of the world that enables humankind to become nature's final end.

The view of the relationship between the rational intelligence and perception rejects an uncontrolled imagination, which Kant shows, in his discussions of hypochondria, can be used to depict ways of life that are contrary to the real conditions of experience explained by our rationally grounded perceptions. In this regard, Kant discusses Swedenborg, who claims to communicate with the souls of deceased people. Shell says that, for Kant, Swedenborg is a "dreamer" whose "world is in principle one that cannot be shared by others" (p. 126). The dreamer's private world, because it disregards the place of the rational intelligence, would not provide the living with a basis for a community that embodies the moral law.

In this type of community, each person is accorded dignity and thus lives only by the moral norms that rational people accept. A community practically embodies this objective as a civil society with a republican form of government, which secures private property, the rule of law, and the rights of each person. Kant's ethics, then, as Shell rightly points out, is not the "empty formalism" Hegel attributed to it (p. 146). Indeed, Kant's view of history supports the argument that nature is designed, as it pertains to the human species, to realize a "community of moral beings" (p. 175). Here, human beings become the ultimate end and the highest good of nature.

A problem remains, however. As Shell demonstrates, in Kant, to uphold humankind as the final end of nature, it is necessary to envision nature "as if" it were designed to achieve this objective. But by doing so, the unity between the rational intelligence and nature is based upon the assertion that humankind is the final end, an assertion which, while reasonable and beyond dispute, cannot be derived from our experience with nature. This experience, which suggests that nature is filled with "boundless heterogeneity" marked by cycles of life and death (p. 206), does not support the view that humankind is nature's final end. Still, by associating nature with the idea of humankind as the final end, it is possible to show how nature's heterogeneity can be useful in achieving this great objective. Thus, Kant argues for a culture of rational beliefs, a culture characterized by faith in reason's (or the rational intelligence's) idea of humankind as the final end of nature and as a species capable of living in a society governed by the moral law. Indeed, Kant turns to belief in the existence of God, "an intelligent first cause," to maintain humankind's status as the highest good and the final end of nature, within a community which makes this objective supreme (p. 198). Shell is skeptical of the success of this endeavor, however, and she says, "in the end, Kant blinks, unable (without recourse to God) to disabuse himself of the disabling thought that the idea of the highest good . . . is a chimera" (p. 261).

Shell, in discussing the possibilities of Kantian community for bridging nature and humankind, omits an important element. Kant's world view can be seen as containing an approach to communication that allows individuals to form the communal setting that secures respect for the moral law. As Onora O'Neill argues ("The Public Use of Reason," *Political Theory* (November 1986): 533–4; 543–5), Kantian morality protects the dignity of each person by making possible a type of communication which uses commonly accepted standards of reasoning in order to encourage mutual respect for difference, while securing autonomy of thought and judgment. Shell's account of Kant's definition of the world, in this important contribution to Kant studies, needs to build upon this insight to provide a complete definition of Kant's world view.

Edmund Burke: Modernity, Politics, and Aesthetics. By Stephen K. White. *Modernity and Political Thought*, vol. 5. Thousand Oaks, CA: Sage Publications, 1994. 107p. $42.00 cloth, $18.95 paper.

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Edmund Burke. The name seems strangely out of place in our "post–cold war" times. Indeed, with the collapse of communism, says Stephen White, this enemy of "political rationalism and radicalism" would appear to have little to say on "the issues that animate us at the close of the twentieth century" (p. 1). Complicating the question of Burke's relevance for contemporary readers is his notoriously rambling, affect-laden prose and the absence of a "major philosophical treatise" (p. 2) in his wide-ranging oeuvre. In light of these difficulties, we may well wonder whether a return to Burke can provide any insight into the problems of (post)modernity.

Opening with this question, White offers, if not a definitive yes, a strongly argued case for the rewards of rethinking Burke free of the ideological antinomies that structured cold-war political debate. Resisting a more current trend of interpretation, White also shows the difficulties faced by neoconservatives who find in Burke's writings theoretical ammunition for their stance in the current "culture wars." White interrupts this and similar appropriations of Burke, reminding us of the pieces that do not fit into the traditional interpretive frame, for example, the fact that this so-called father of conservatism "went out of his way to plead leniency in the criminal punishment of homosexuals" (p. 2).

Notwithstanding the dominant image of Burke as the irrepressible spokesman of tradition and the Old Regime, Burkean interpretation, as White shows, has been fraught by contradictory accounts of the "real Burke." For eighteenth-century critics, like Thomas Paine, Burke was an ally of the landed aristocracy; for nineteenth-century critics, like John Morley, Burke was a liberal constitutionalist; for twentieth-century critics, like C. B. Macpherson, Burke was a defender of capitalist accumulation. White does not try to adjudicate the case of Burkean interpretation but, rather, considers the possibility that each reading has a measure of legitimacy inasmuch as Burke's "arguments are often couched in more than one idiom or language game" (p. 2). Although White is attentive to this linguistic plurality, he rightly insists that certain themes and concerns persist throughout Burke's writings and that these converge in his complex critique of what White calls "political modernity."

For Burke, political modernity entailed a "systematic explosion of willfulness in politics that had a world historical significance" (p. 5). Culminating in the French Revolution, the sheer force of political modernity, says White, was evident to Burke as early as 1780 in the form of the Gordon Riots. More backward than forward looking—a violent popular response to parliamentary moves for increased toleration of Catholics in England—the riots were nevertheless an example of something new in politics: a volatile mixture of increased popular agitation and a diminished sense of human limits. Attending to Burke's less well-known writings and the political events that provoked them, such as the Gordon Riots, White enables us to see the complexity of Burke's thought, which tends to get lost in interpretations that focus exclusively on his response to the French Revolution.

Central to White's thoughtful rereading of Burke is a fascinating account of Burke's 1757 essay on aesthetics, *Enquiry into the Origins of Our Ideas of the Sublime and the Beautiful.* This text, says White, offers an interpretive angle from which to consider the range of Burke's political reflections, allowing us to "separate the wisdom in Burke's work from its reactionary excesses and blindnesses" (p. 83). What White finds and applauds in the *Enquiry* is the articulation of an "aesthetic-affective dynamic," which emphasizes the importance of feeling or sentiment in political judgment. Crucial to this dynamic is Burke's account of the sublime. According to White, the Burkean sublime's "deepest significance as a category of human experience resides in the way that it confronts us with human finitude" (p. 30). White argues that it is this sense of limits which unravels in the French Revolution. But he is equally concerned to show—in stark contrast to those critics who would demonstrate a direct link between the *Enquiry* and the *Reflections*—that the "interplay between [Burke's] aesthetic and political ideas" (p. 41) has a less direct and more complex genealogy. By attending to the development of Burke's aesthetic-affective dynamic in his "middle period," White enriches our understanding of how aesthetics came to structure the whole of Burke's critique of political modernity.

Through careful readings of Burke's writings both before 1780 and between 1780 and 1790, White develops an acute analysis of the place of the social affections in Burke's thought. As Burke came increasingly to see the importance of the sentiments in nourishing human sociability, he altered his 1757 account both of the sublime, as that which inspires awe and dread in subjects, as well as the beautiful, as that which is of significantly less momentous concern for political society. On the altered account, the authentic sublime comes to figure, not the exercise of limitless power, but the power of limits; the beautiful comes to figure, not the window-dressing of sociability, but the glue of social affection. Although White is not the first critic to notice this shift in Burke's political aesthetics, his reading is especially valuable for its attentiveness to Burke's gradual reformulation of the aesthetic-affective dynamic as he responded to a variety of historical and political events (such as the American Revolution, the Gordon Riots, and the crisis of British India under Warren Hastings). What becomes visible in White's account, in other words, is the extent to which abstract theoretical categories, like the beautiful and the sublime, take on different meanings for Burke in the context of shifting political practices.

Tracing this change in Burke's thinking over the course of three decades, White better prepares us for understanding the place of aesthetics in Burke's *Reflections on the Revolution in France*, that is, for making *political* sense of the aesthetic concepts of the sublime and the beautiful that organize Burke's attack on Jacobinism. This is a valuable and important contribution that both builds on and departs from the existing literature in its insistence that a series of political events, and not just the French Revolution, shaped Burke's aesthetic-affective dynamic.

Where White is somewhat less successful is in helping us to think politically about the excesses of Burke's prose, especially those places in the *Reflections* where language both exceeds its ostensible empirical referent and threatens to collapse under the weight of authorial affect. In some sense, this less satisfying aspect of White's reading is the other face of the real strengths of his interpretive strategy. Every reader of Burke inevitably comes up against his almost hysterical flights of rhetoric, most notably the dramatic account of the Jacobin assault on Marie Antoinette. Because White wants to elaborate a Burke who is both more consistent in his critique of political modernity and more relevant for late-twentieth-century readers, he understandably chooses to emphasize those aspects of Burke's thought that lend themselves to a more prudent assessment of his legacy.

One example of how this choice may limit our understanding of the politics of Burkean rhetoric is White's interesting account of Burke's response to a 1789 letter written by Madame Parisot, a prominent Frenchwoman with whose family the Burkes were well acquainted. The letter, which describes in vivid detail the family's "plight during the 'Great Fear' " (p. 64), says White, "was not, for Burke, just one more piece of political news, but rather an aesthetic-affective jolt, on the basis of which he could thenceforth speak definitively about France" (p. 65). White cleverly uses this letter to establish a context in which to make sense of what Paine called Burke's "horrid pictures," suggesting that it was this earlier (and for Burke more personal) scene of the humiliation of women that elicited the author's fury and inspired his chivalric defense of the queen. "When one focuses instead, as is usually the case, on the more famous scene of Marie Antoinette's distress," White remarks, "this continuity becomes overshadowed by the hyperbole and histrionics of Burke's description of that event" (p. 66). Stated somewhat differently, as White acknowledges, Burke's critique of political modernity is compromised by the reader's sense that the author has simply lost his mind.

Perhaps there are other means for thinking politically about Burke's rhetoric, and especially his figuration of women as the helpless victims (and active agents) of what White calls the "false sublime," the transgression of human limits and the sense of "human infinitude" (p. 84). White himself provides us with these means when he argues that Burke's central and enduring contribution to political thought lies in his trenchant critique of politics without limits. Following White's lead, we might push farther to ask: How do certain conceptions of the *feminine* work to sustain our sense of the authentic sublime or a sense of limits? What happens when those conceptions are put into question? If we reject, as White says we must, both Burke's oppressive understanding of gender and his notion of a natural hierarchy to provide such limits, can we refigure the sense of human finitude that White persuasively argues is crucial to the future of democratic cultures? Suggesting that a micropolitics of the self, rooted in an aesthetic sensibility, may frustrate the "modern imperatives of rationalization, especially the unlimited drive for mastery" (p. 84), White modifies Burke in order to recover from him an important lesson. What we make of this lesson may depend on hearing the (sexual) difference in the question that White puts to us in his concluding chapter: "How does *any* individual cultivate *for herself* an authentic sense of the sublime (p. 85)?"

Labyrinths: Explorations in the Critical History of Ideas. By Richard Wolin. Amherst: University of Massachusetts Press, 1995. 290p. $50.00 cloth, $16.95 paper.

Gregory Bruce Smith, *Trinity College*

In *Labyrinths*, Richard Wolin reprints a series of previously published essays on the general topic of the nineteenth- and twentieth-century critiques of reason. Like so many, he sees the excesses inherent in the modern, instrumental understanding of reason, and he admits the power of many of the critiques of that understanding, especially those that descend from the thought of Nietzsche and Heidegger. But he also sees the dark underside that lurks in those critiques. Hence, like many before him, he points toward the need to resuscitate a conception of reason that can still make critical, ethical, and political judgments. That undertaking is both noble and essential. But it is far easier to explain the need than it is to supply what is needed.

Wolin does a good job of tracing the transformations and deflections of Heidegger's thought, his principal exemplar of the "total critique of reason," as it migrates to France and emerges as "left Heideggerianism." He is also helpful in tracing the reception of that left Heideggerianism in North America. But he goes beyond these efforts in an attempt to link Heidegger's thought with contemporaries in Germany whom he calls "radical conservatives," such as Junger, Spengler, Gehlen, Klages, and Schmitt. He argues that what they all share is a critique of bourgeois civilization and therewith a rejection of reason and democracy. They allegedly also depict an "end of History" where there is no longer any place for nobility, true passion, intensity, or heroism. Wolin gives no indication that there is anything philosophically deeper going on in the thought of Heidegger. He goes on to argue that acceptance of the end of History theme rematerializes when the German critique migrates into French postmodernism, especially in its Derridean manifestation, albeit now the end of History is seen as the more ambiguous deliverance from the tyranny of rationalist "metanarratives." But once again, reason is criticized, and with it, according to Wolin, the possibility of democracy.

Wolin argues that the total critique of reason that descends from Heidegger and the German radical conservatives, even when passed through the lens of French left Heideggerianism, makes democracy impossible. For Wolin, democracy has only one foundation, the Enlightenment understanding of reason, which he traces to the spirit of 1789. He includes in this tradition Kant, Marx, Habermas, and what he vaguely designates as a post-Marxist theory of democracy that is fundamentally critical of capitalism (p. 236). What they all allegedly share is an abstract, universalist understanding of reason. Without this, we would allegedly have the end of History, and with it the "end of meaning."

The most substantial part of the book is to be found in the historical analyses which include not only tracing the roots of French postmodernism but also discussions of intellectual phenomena such as the Kulchur wars, Jewish secular messianism, the Heidegger affair and the French Heidegger wars, "deconstructing Auschwitz," and the recent debates over the status of humanism. As regards the latter, French Heideggerians like Derrida and Lacoue-Labarthe have argued that Nazism is a form of humanism and that its excesses follow from an over reliance on an abstract conception of reason qua Will that is shared with the Enlightenment. This cuts to the heart of the matter with which Wolin is dealing. But he merely asserts in opposition that the real humanism is to be traced to the abstract tradition of reason he favors and that Nazism is therefore an anti-humanism. In their favor, the left Heideggerians can point to how many innocents have been slaughtered in this century in the name of the abstract universal in Russia and China alone. How the modern understanding of reason is implicated in those atrocities requires far more interrogation than Wolin gives it.

Given that Wolin sees no possibility of an understanding of reason that lies outside the modern Enlightenment project, he seems forced into various either/or categories. For example, either we choose the abstract, universal conception of reason or the total critique of reason. A more nuanced account would recognize that there are options between these two poles. But for Wolin there are apparently only two alternatives available. And while he seems to admit that there are problems involved in the modern hegemony of instrumental reason and with the creeping conformism of modern, global civilization, given the substance of his argument, these seem to be no more than throw-away lines.

Rather than recognize the difficulty involved in the philosophical undertaking he supplies, we get optimistic assertions that there are "countervailing forces" to be found in politics, art, and everyday life that can easily counteract the dangerous tendencies of an abstract, universalist understanding of reason. All we are given as examples is a vague gesture in the direction of unnamed social protest movements. In place of serious philosophical analysis, we even get the curious observation that the left Heideggerians were not as extreme as the Germans in their critique of reason because they were Jews: "Perhaps it was their Jewishness, their subterranean, seldom avowed affinities to "rational religion" and the taboo against images, that prevented them from taking the final step of casting off civilizing reason in favor of myth" (p. 11).

That Wolin has, in the last analysis, primarily restated rather than solved the predicament of reason in our time—albeit at times with useful clarity—is not surprising. It is likely that this predicament will remain with us for some time to come. Clear statements of the issues—which Wolin offers in places—are quite valuable. Vague rhetorical gestures to already available philosophical solutions are not.

When Wolin goes beyond historical treatments and synopses of the ideas of various authors he stumbles in significant ways. The seeming inability to see the philosophical incommensurability between the thought of Nietzsche and Heidegger, on the one hand, and such authors as Junger, Schmitt, Klages, and the left Heideggerians, on the other, weakens the argument. Easy victories over derivative thinkers will not bring us to the heart of the problem. In this vein, despite castigating both the right and the left for being overly polemical, Wolin falls into the same trap. He justly warns us about the extreme, if paradoxical, dogmatism of Derrida and his deconstructionist priests, as well as the antidemocratic tendencies of many of the authors with whom he deals. But this undertaking cannot be confused with the level of philosophical analysis that is needed to deal adequately with thinkers of the magnitude of Nietzsche and Heidegger. It does not even make for very compelling polemics.

To point to the intellectual lineage of ideas, or to show overlaps with the ideas of easily indictable secondary thinkers, is not the same as being able to confront philosophically serious ideas in a fundamental fashion. The same is true of merely pointing to what are alleged to be the necessary, concrete ramifications of those ideas, especially when primarily dealing with a permutation offered by derivative thinkers. There is no one-to-one correspondence between ideas and practical consequences—reality is much more complicated than that. Furthermore, ideas may have certain antecedents and ramifications and still be simply true, or so much more powerful than anything else available that they are presently unassailable. The all too facile jump from political and historical analyses to philosophical conclusions can never take the place of actual philosophical analysis. Unfortunately, we see all too many of these kinds of analyses masquerading as serious thought—in this regard, Stephen Holmes's recent *The Anatomy of Antiliberalism* (1993) comes immediately to mind.

Serious philosophical analysis requires that one be able to bracket one's moral and political commitments and indignation. *That is what reason truly requires.* If that distancing is not possible, rhetoric about recovering reason is absolutely trivial. What Wolin designates as the "effective history of ideas" carries with it far too many presuppositions and political commitments to be of philosophical interest; many of those presuppositions are ultimately shared with his primary antagonist, Heidegger. If others wish to make a political and moral leap of faith, or engage in the same act of ideological will as Wolin, so be it. But there should be no confusion about that being an act of reason.

It is not surprising that when Wolin points toward a concrete alternative to the body of literature he is discussing, it consists of no more than two citations of authors who are alleged to be at the forefront of a massive new movement in democratic theory. And this is conjoined with warm remembrances of the protest movements of the 1960s. This all gives the appearance of flag waving in an attempt to certify one's pedigree and attest to the impeccable nature of one's own antecedents. Either that or we are being offered no more than easy-going intellectual nostalgia in the face of a powerful philosophical critique of reason. Many of us felt the exhilaration of the 1960s. But philosophically that is neither here nor there unless we are prepared to accept simple ideological commitment and engagement as our solution and eschew serious philosophical analysis. In fact, that is precisely Heidegger's conclusion: In the absence of a ground for reason, committed resoluteness is all that is available to us.

American Politics

The New South, 1945–1980. By Neuman V. Bartley. Baton Rouge: Louisiana State University Press and The Littlefield Fund for Southern History of The University of Texas, 1995. 548p. $39.95.

Charles D. Hadley, *University of New Orleans*

This is the eleventh volume in a series published since 1949 by LSU and the Littlefield Fund to chronicle the history of the South. Although the book deals principally with the 1945–1980 period, Bartley reaches back to explain the combined effect of the Great Depression, New Deal, and World War II on the region because, together, they set in motion the economic and social forces that eventually transformed the South and its politics. He also extends his thorough and well-documented research into the early 1980s. His organizing themes are economic change, race—the integration of blacks into all facets of southern life enjoyed by whites regardless of socioeconomic position—and conservative white reaction to it.

Why is *The New South, 1945–1980* of interest to political scientists? It is a comprehensive volume that explains the transformations of the South from a rural agrarian to urban industrial society, from a segregated caste-based society to an integrated one, from an uneducated to educated society, from a region of extremely low voter turnout coupled with rock-solid support for Democratic candidates at all levels of government and politics to voter turnout not unlike the rest of the country but with an increasing affinity for Republican candidates, especially in presidential politics. The book's strength is its comprehensiveness. Most books deal with a specific aspect of the South and/or its politics, such as the civil rights movement. Or they focus more narrowly on an important organization, community, event, or piece of legislation, such as the Southern Christian Leadership Conference (SCLC), Student Nonviolent Coordinating Committee (SNCC), Tuskeege or Selma, Alabama, *Brown* v. *Board of Education*, the Civil Rights Act of 1964, or the Voting Rights Act of 1965.

This book takes a broad view; Bartley carefully explains how each event was driven by economic change which increasingly forced the integration of blacks (and poor whites) into the mainstream, in turn producing reactions among racially conservative whites. This interaction systematically produced pressures to change life in the South, pressures that eventually led to a complete transformation of the region and its politics. In addition, the book has an extensive topically organized "Critical Essay on Authorities" (pp. 471–503), which is an excellent starting place for future research focusing on the South and its politics.

The themes of the volume are established in the first chapter. There Bartley writes that the 1938 *Report on Economic Conditions of the South* identified the region as backward, one of the "poorest . . . in the Western world" (p. 2), compelling President Franklin D. Roosevelt to consider it the most important economic problem in the country. Its black citizens were at the very bottom and competed with poor whites to set a low-wage standard for the region that made it "an economic colony of the Northeast" (p. 3). Moreover, because the South was the backbone of Democratic control of Congress, World War II brought the training of massive numbers of soldiers and industry to support the war effort to the region. Industry and its wages drew workers away from the rural to urban areas. Black soldiers and others from outside the South saw German prisoners of war treated far better than Southern blacks. Often lacking enforcement powers and/or will, agencies of the national government were lax toward Roosevelt's executive order to end discrimination in defense industry employment and training programs. Nevertheless, economic and social change was in motion. It intensified after the war with the return of military personnel, who challenged and broke the monopoly of courthouse politicians, challenged "separate but equal" education with the exercise of GI Bill benefits, and increasingly challenged the traditional social system maintaining segregation. World War II, some feel, had more effect on the region than did the Civil War.

To understand southern politics is to understand "The Rise and Fall of Postwar Liberalism," which led to "The Dixiecrats and Southern Conservatism" (chapters 2 and 3). Southern liberalism related to organized labor, especially the efforts of the Congress of Industrial Organizations (CIO) in voter registration and turnout with an eye toward the election of progressives. The CIO invested considerable funds and effort to organize and mobilize black *and* white workers through Operation Dixie, an undertaking that collapsed with the failed presidential bid of Progressive Party candidate Henry A. Wallace in 1948. That same year, conservative white Democrats rallied around the Dixiecratic movement, which eventually chose Governor Strom Thurmond of South Carolina as its presidential candidate, an event that marked the beginning of the end of the Democratic Solid South.

"The Making of the Modern South" (chapter 4) occurred through a combination of population and economic movement. The region's people moved from farms to cities and out of the South; for example, between 1940 and 1950, the farm population dropped from 42% to 15%, and 20% of southerners lived outside the region by 1960. The cotton belt moved from the Deep South to a new area that stretched from western Texas to southern California, and industry moved South. Atlanta, Dallas, and Houston replaced New Orleans as commercial centers.

Chapters 5–10 (pp. 147–380) deal with different aspects of race in the life of the South, especially the events that led to the civil rights movement and its aftermath. With that said, chapter 5, "Race and Reform," gives attention to the transformation of southern society and the changing place of its black population; chapter 8, "God and Society in the Modernizing South," gives attention to the transformation of religion in society with respect to the creation of new black and white middle classes and the politics of moderation. In effect, the modernizing South was composed of four societies—two working classes and two new middle classes, each with one black and one white segment.

Chapters 11 and 12, "Politics, Protest, and Palliative" and "The Sunbelt South," explore the modern South, the unfolding of the region from the mid-1960s through the 1970s. Senator Barry M. Goldwater oriented his 1964 presidential campaign around issues near and dear to conservative white southerners and opened the door for the election of Richard M. Nixon as president in 1968. Republicans remained on the move in the region, though Democrats responded with electable "new South" moderate candidates for statewide office. Blacks, with the aid of the Voting Rights Act of 1965, came into the mainstream of southern political and social life, as did women with the Civil Rights Act of 1964. In the 1960s, moreover, southerners stopped moving out of the region, and well-educated whites began moving in, especially to the states of Virginia, Florida, and Texas. By 1980 20% of the region's population was born outside the probusiness South.

The themes of *The New South, 1945–1980* are brought together in the final chapter. The book is a "must read" for a diverse audience of political scientists, from southern, black, and state politics scholars to those with an interest in national politics or the economic and social changes that work to transform politics.

The Kingfish and the Constitution: Huey Long, the First Amendment, and the Emergence of Modern Press Freedom in America. By Richard C. Cortner. Westport, CT: Greenwood Press, 1996. 196p. $55.00.

Philip A. Dynia, *Loyola University New Orleans*

To introduce students to the subject of the incorporation of the Bill of Rights into the Fourteenth Amendment and the subsequent application of most of those rights to state governments, this reviewer has often presented his class with the following "hypothetical": Let us assume that "the solons in Baton Rouge" (the quaint nineteenth-century appellation for legislators that one New Orleans newspaper regularly used in its headlines well into the 1970s) passed a tax on the gross advertising receipts of newspapers in Louisiana, but limited the tax's applicability to 13 newspapers out of a total of 163 in the state. Of these 13, 12 were active in their editorial opposition to the dominant political powers in the state which totally controlled the legislature that passed the tax. Is the U.S. Constitution violated? Which provision?

Instinctively, even the rawest political science freshlings (a colleague's politically correct term for first-year students) know that the Constitution is indeed violated, and most believe it is the First Amendment's guarantee of freedom of the press that has been transgressed. When I tell them that the Fourteenth Amendment is violated, they are puzzled. We then read the Fourteenth aloud, some may mutter (not incorrectly, but prematurely) "equal protection," and thus begins our discussion of the nationalization of the Bill of Rights.

Like many of their fellow citizens, these students know very well that the First Amendment applies to *all* levels of government, but they do not know very well the details of the half-century process by which that felicitous (*pace* Edwin Meese) contemporary reality came about. The answer to the question was not so easy or self-evident in the early 1930s, however. The case in which the Supreme Court began to address the issue is the subject of Richard Cortner's book, a fascinating, richly detailed study of *Grosjean* v. *American Press Co.* (297 U.S. 233 [1936]), whose essential facts are the basis for the "hypothetical" above.

Cortner's book is another example of a familiar genre (e.g., Anthony Lewis, *Gideon's Trumpet*, 1964, and *Make No Law*, 1991; Richard Kluger, *Simple Justice*, 1976; and Marian Faux, *Roe V. Wade*, 1988)—the in-depth analysis of a "landmark" Supreme Court decision. Indeed, Cortner himself has done much over the years to make the genre so familiar, with *The Wagner Act Cases* (1964), *The Apportionment Cases* (1970), *A "Scottsboro" Case in Mississippi* (1986), and *A Mob Intent on Death* (1987). The requirements of the genre are all present in *The Kingfish*: a sketch of the development of the law before the landmark decision, an account of how the particular dispute arose (including portraits of some of the key actors), the legal arguments and strategies developed by the parties, the actions taken by lower courts, the decision and opinion of the Supreme Court, and finally some assessment of the case's significance.

Half the book is devoted to the conflict between Huey P. Long and the opposition press (primarily the urban dailies). Cortner's account is painstakingly detailed and includes some original research (Cortner read every edition of the Long-controlled *Louisiana*—later *National—Progress*, an unenviable albeit necessary task). These first hundred pages sometimes seem like overkill; we are given many, many quotations from the flamboyant and outrageous rhetoric characteristic of both sides. Few would doubt by page 90 what Cortner said at the outset: Governor Long despised the Louisiana (and national) press, and his feelings were reciprocated. This first half concludes with an entertaining account of the infamous 1934 legislative session, dominated by (now U.S. Senator) Long, which enacted the tax described above. Cortner captures the color, excitement, buffoonery, and ultimately great danger of Long and his era (elements of which persist in Louisiana).

The remainder of the book details the newspapers' decision to contest the tax, the arguments and strategies developed by their attorneys and the defense of the tax's constitutionality concocted by the state, the arguments before a special three-judge district court, its decision, the appeal by the state, and the ultimate resolution by the Supreme Court. Cortner is firm in his grasp of the details, and his discussion of procedural and substantive issues in the case is lucid and thorough—if somewhat repetitive (the basic arguments of each side did not change dramatically, but Cortner tells us what they were before the trial, again at the trial, and again on appeal).

Some may quibble with his characterization of *Grosjean* as a "landmark"; though mentioned, it is not excerpted in Gerald Gunther's *Constitutional Law* (10th ed., 1991), and Laurence H. Tribe, in *American Constitutional Law* (2d ed., 1988) cites it five times, compared to thirty-four citations for *New York Times Co.* v. *Sullivan*. Yet, arguably it is an important case. It came on the heels of *Near* v. *Minnesota* (283 U.S. 697 [1931]), in which the Court for the first time held that freedom of the press is protected by the Fourteenth Amendment against state infringement. The *Near* Court split 5–4, even though the case dealt with a flagrant example of prior restraint. The question remained very open as to whether the Court would read freedom of speech and press as something broader than simply a ban on prior restraints. The unanimous decision in *Grosjean* answered that question affirmatively and helped pave the way for much of contemporary First Amendment law. It is especially interesting to learn from Cortner that the Court initially voted to invalidate the tax on equal protection grounds, with the majority opinion assigned to Justice Sutherland. Justice Cardozo, in a concurring opinion, argued that the tax should be overturned as a clear violation of freedom of the press. Cardozo's concurrence was so persuasive that Sutherland rewrote his opinion to incorporate most of Cardozo's free-press argument, and that redrafted opinion ultimately won the support of all the justices. Sutherland's opinion followed extremely closely the free-press arguments outlined in a strategy memo written shortly after the tax was enacted. The memo's author, a brilliant and prescient attorney, Eberhard Deutsch, argued the case before the three-judge district court but was not chosen to argue before the Supreme Court. His views, however, were detailed in an appendix to the brief filed by the newspapers.

If not a landmark, *Grosjean* remains a case worth recalling, and Cortner's study allows us to do so enjoyably.

America Unequal. By Sheldon Danziger and Peter Gottschalk. Cambridge, MA: Harvard University Press, 1995. 220p. $26.00.

William Kelso, *University of Florida*

Sheldon Danziger and Peter Gottschalk present a rather bleak picture of the U.S. economy in this provocative book. They maintain that since 1973 there has been an overall slowdown in the growth of wages for the average U.S. family as well as a growing polarization of income between the haves and have nots. Danziger and Gottschalk also insist that the reasons for the growing disparity of income in this country are primarily structural or economic. The growth of a high-tech global economy has dramatically reduced the demand for unskilled workers, thus depressing the wages of high school graduates. They dismiss the possibility that problems such as the breakdown of indigent families or the erosion of cultural values may be exacerbating the growing polarization of income in society. Finally, the authors conclude there is little possibility that either the marketplace or government can correct these problems. At best the government can treat the effects rather than eliminate the causes of poverty. They call for major increases in such redistributional programs as the earned income tax credit, day care credits, and public service employment to supplement the wages of the working poor and enhance their purchasing power. As underscored by the title of their book, Danziger and Gottschalk believe not only that incomes have become more unequal but also that the United States has become unequal to the task of eliminating the causes of wage polarization.

Unfortunately, Danziger and Gottschalk may be overstating the seriousness of the problem. While they are correct that wages have risen little since 1973, they fail to mention the fact that employers have increasingly given their workers noncash compensation which is not counted in income data. Between 1948 and 1994, nonwage compensation rose from 5% of the average employee's total pay package to 16%. Even more important, it now appears that the falling wage thesis has been based on inaccurate inflation numbers. A recent commission created by the Senate and headed by Michael Boskin, former chairman of the Council of Economic Advisors, concludes that the Department of Labor's inflation figures have been too high. Based on Boskin's new calculated rates, it turns out that earnings for male workers did not fall by 12% over the last two decades but instead rose by 14%.

In addition, Danziger and Gottschalk can be faulted for considering only a limited number of explanations for the persistence of poverty in this country. For instance, they never seriously discuss the possibility that a breakdown of cultural values—in addition to structural factors—may be responsible for the plight of the poor. They ignore and never really engage scholars like Myron Magnet and Lawrence Mead, who insist that the United States may be developing an underclass that refuses to subscribe to conventional norms of behavior, such as actively looking for work. Surprisingly enough, the "underclass" is a subject that appears nowhere in this book. As a result, the authors often give the misleading impression that all the poor are working individuals who just happen to be suffering from the scourge of low wages. Census data indicate, however, that just 10% of the poor work full time, full year, while almost 46% of the nonpoor are full-time workers. If we look at women on welfare, the picture becomes even more bleak. The House Ways and Means Committee reports that only 2.2% of mothers receiving AFDC work full time, full year. The above data certainly indicate that one reason the poor are so much worse off than the rest of society is that they are less inclined to work.

Unfortunately, the problem has become much more severe since the 1950s. A review of the employment record of poor families, who make up two-thirds of the poor, indicates there was a drop of 30% in the number of poor people who worked full time, full year between 1959 and 1973. This significant decline in the work record of the poor occurred a decade before the structural factors Danziger and Gottschalk insist are the primary causes of poverty. Because they also ignore ethnographic data, such as the University of Chicago's Urban Poverty and Family Structure survey, they never grapple with the fact that the poor appear to have a wide array of attitudes toward work. The above survey found that many inner-city residents believe the spotty employment practices of their peers reflect more a lack of character than a lack of opportunities for advancement.

While Danziger and Gottschalk ably illustrate the problems facing the working poor, they fail to mention, let alone discuss, the equally serious problems of the underclass who choose not to work at all. Poverty in this country is really a dual problem. The economic difficulties of the working poor, which are essentially the subject of *American Unequal*, are very different from the cultural and family dilemmas plaguing the underclass.

Finally, in reaching their rather pessimistic conclusions about the U.S. economy, Danziger and Gottschalk may have failed to use a long enough time horizon. Their focus on the last twenty years may be an inadequate period to conclude that the economic prospects for the poor are getting worse. After all, the United States is about as "equal" today as it was in 1945. Furthermore, many Americans have flourished during the time our authors complain about. For example, between 1970 and 1990, the median Japanese-American family saw its income jump from 127% to 146% of median family income. Japanese-Americans, like many other Asian and East Indian families in this country, did well because they had relatively stable families and a culture that stressed the importance of education and entrepreneurial activities. If these ethnic groups did well, perhaps we can learn something from their achievements and apply it to the population as a whole. But to reach that objective, we would have to focus on the importance of such topics as culture, values, and family structure, issues which Danziger and Gottschalk think are relatively unimportant. Even if we concentrate just on the economic plight of the working poor, the prospects for improvement may not be as bleak as the authors suggest. Over the last two decades, increases in productivity have slowed dramatically from the decades following World War II. Generally, higher wages have tracked higher worker productivity. Perhaps government policies which encourage more capital investment may eventually raise productivity and thus lead to higher wages for working Americans.

Yet, if Danziger and Gottschalk are correct that there is little the government can do to eliminate the causes of growing inequality, the political consequences for the country may be severe. Instead of giving the poor a helping hand, Danziger and Gottschalk essentially advocate giving them a more equal share of the services and goods of the U.S. economy. The danger thus exists that the politics of reconciliation may give way to a more intense form of redistributional and class politics, in which the Pat Buchanans of the right and left mobilize the poor and working classes against the well-to-do. If the national economy does become increasingly unequal, we must realize that U.S. politics may also become increasingly contentious and polarized.

What Americans Know about Politics and Why It Matters. By Michael X. Delli Carpini and Scott Keeter. New Haven: Yale University Press, 1996. 397p. $38.50.

Lonna Rae Atkeson, *University of New Mexico*

Since the beginning of democratic thought, scholars have debated the ability of citizens to live up to the demands of a democratic society. In general, citizens are expected to equate their self-interest with their actions in the political arena. Since the behavioral revolution and the introduction of survey research, however, scholars have confirmed over and over again that the public comes up short. Many people are unaware of and unknowledgeable about even the most basic rules governing politics as well as the beliefs and policy positions of leaders, and they often have limited knowledge of current events. Many scholars have addressed this tension between theory and practice by redefining democracy and redefining what citizens should know. Delli Carpini and Keeter, however, offer compelling analysis and argument that the answer to the tension between democratic theory and practice is to be found in working toward a more informed citizenry. They believe, as their title indicates, that information matters and that the more knowledge citizens bring to bear on politics, the better they are in linking their political behavior with their political attitudes, and ultimately society is better off for it. Since I can think of very few political scientists or political pundits who would oppose a more knowledgeable citizenry, the normative framework of this book fits well with conventional goals.

The book covers all the major areas of public knowledge. It examines what citizens know and do not know about politics, compares the knowledge of today with that of the 1940s and 1950s, examines who is informed and about what, and explores what factors explain political knowledge. Much of this information is consistent with previous studies. At most, roughly half the public is knowledgeable about political life, and more than four decades of public opinion research indicates no substantial change in the amount of political information citizens know. The authors reveal that citizens are generalists, and the more they know in one area, the more likely they are to know more in another area. All citizen are clearly not equal, however. Those with more resources and traditionally more access to government (men, whites, the wealthy, etc.) are likely to be more informed. Last, they find that both structural and behavioral factors help to explain citizen political knowledge.

I found chapter six, "The Consequences of Political Knowledge and Ignorance," the most interesting and most supportive of the authors' normative goal of a more informed public. They explore a number of different areas in this chapter, including the relationship between political knowledge and political tolerance, political participation, stability of attitudes and connecting citizen self-interest with attitudes, vote choice, and partisanship. The authors thoroughly examine how political knowledge allows people better to connect their self-interest with their political behavior and attitudes. The conclusion is that more informed citizens are more likely to behave in ways consistent with their policy beliefs.

From the beginning, the authors try to put a positive spin on their findings. For example, they note that over time the public does learn new information. They view this learning as hopeful and as evidence that an informed electorate is possible. Yet, this hopefulness is often manifest as an apparent overstatement of their findings. It is true that the public learns over time about a few specific issues, policies, and leaders, but that does not necessarily mean it is realistic to believe a more broadly defined "informed public" is possible. Their normative argument rests on the belief that the more political knowledge the public has the better; although I would like to see a more informed public and am sympathetic to their argument, I do not find the few cases of increasing knowledge in specific areas cause for overwhelming optimism.

One failing of the book is that the reader is never given a sense of whether or when a democracy is healthy. By focusing on the idea that a more informed public is always better, the authors never have to face the tough question of how much of the electorate needs to be informed to attain a more, as opposed to a less, democratic state. They note that about one-quarter of the U.S. public is informed and behaves in patterns consistent with the democratic ideal. But they never answer or even address the question of what portion of the electorate would need to be informed in order for U.S. democracy to perform adequately. This allows them to be upbeat and optimistic throughout, although I often wondered whether their confidence was misplaced.

In the end, the authors try to translate their findings into arguments for increasing public knowledge and suggest many ways they believe that is possible. They argue that three factors are most important in understanding overall citizen knowledge: the ability of the citizen, the opportunity the citizen has to obtain information, and the motivation of the citizen to seek out information. Their remedies, however, focus largely on ways the system can change in order to promote citizen interest and accessibility. Many of these seem unlikely, and therefore the authors fall short in providing reasonable expectations about how we can change the environment to achieve a more informed citizenry. In addition, since overall political knowledge has changed little over fifty years, it seems difficult to prescribe "fixes" to the system that will not be offset by a continually changing technological environment.

Overall, the book is a good synthesis of the state of political knowledge and research on the subject. The authors attempt to pull together a variety of theoretical discussions about political learning and political sophistication while examining a broad array of public opinion data. They maintain a solid normative framework throughout, providing some new insights and sometimes provocative conclusions. Although many of their findings are consistent with previous studies, their book is useful because of its systematic look at these factors and because it covers a diverse set of research questions in the area of citizen knowledge. Also, the book has compiled numerous descriptive surveys on public knowledge useful for class instruction. In this way, it is a good resource for teachers of public opinion and U.S. government courses and probably for journalists as well. I also liked the fact that the authors propose ways of increasing political knowledge based on their analysis. Although I did not always find those arguments compelling, they offer an interesting place to begin a debate about how to improve public knowledge.

The Ninth Amendment and the Politics of Creative Jurisprudence. By Marshall L. DeRosa. New Brunswick, NJ, and London: Transaction Publishers, 1996. 216p. $29.95.

Michael P. Zuckert, *Carleton College*

It is difficult to state exactly the argument of this book, because it is not clear that it has an argument. What it has, is an attitude. That attitude is a general friendliness to "judicial federalism" and hostility to a Supreme Court jurisprudence of unenumerated rights, to Roscoe Pound and Ronald Dworkin, to the Fourteenth Amendment, and above all to Abraham Lincoln. The argument, such as it is, is largely derivative from the work of M. E. Bradford, late professor of English at the University of Dallas. DeRosa adds little to his mentor's work, and readers interested in a southern conservative perspective on U.S. constitutionalism are advised to consult Bradford's work directly without going through this middleman.

The central theoretic idea DeRosa takes over from Bradford is that of the "nomocratic constitution," a notion "that the constitution was designed to bring government under the rule of law, as opposed to achieving any specific purposes" (cited on p. 150). A constitution that aims to achieve specific purposes is a "teleocratic constitution." DeRosa asserts that the American order is originally supposed to be a nomocratic one, but over the course of U.S. history it has drifted into the teleocratic mode, above all under the influence of the "political fraud," Abraham Lincoln (p. 161). Yet, DeRosa never actually makes the case for this important claim. True, the Constitution mostly does lay out processes, but if one reads the *Federalist* or other sources of the thinking of the framers, it is clear that the processes are for the sake of certain sorts of outcomes. It is no accident, for example, that the *Federalist* begins with a long section of essays (including the important #10) on "the utility of union," that is, the uses or ends served by the new constitutional union. Moreover, even on its face the Constitution does not limit itself to processes. Article I, sections 9 and 10, emphasizes substantive limits, and the Preamble unequivocally states a set of ends the whole structure is meant to achieve.

According to DeRosa, the nomocratic constitution is to find expression above all in "judicial federalism," a concept he declines to define, despite its centrality to his book. He seems to have in mind an arrangement whereby "the sorting out of rights [is] the responsibility of the respective states' political processes and the national amendment process" (p. 8). He is genuinely uncertain about what underlies this strong commitment to federalism. At times, in line with his Bradfordian commitment to the nomocratic constitution, he speaks as if the point is that popular sovereignty expressed by popular majorities at the state level is the only legitimate standard of political action, or the only standard of right there is. Thus, he is unmitigatedly hostile to "higher law" notions or notions of natural rights (e.g., pp. 14–15). Yet, he identifies as "the fundamental principle underlying American constitutionalism" the view that "all governments are inherently oppressive and must be controlled by internal and external checks if the people are to be secure in their lives, liberties, and properties" (p. 141). The first claim is consistent with his notion of a nomocratic constitution, but the second is a teleocratic claim if I ever heard one. This and other inconsistencies receive no adequate recognition, much less resolution.

The sad conclusion is that this is not a very thoughtful book but is a polemic rather than a piece of analysis; it displays the author's attitudes and prejudices aplenty but very little learning or solid argumentation. A good case in point is DeRosa's treatment of the ostensible subject of the book, the Ninth Amendment. For one, it is not really a major theme in the book, just as it has not been a major player in U.S. constitutional history. For another, DeRosa assures us that the historical or authentic meaning of the amendment is plain as day and is supportive of his idea of judicial federalism, yet he nowhere tells us what it actually does mean, nor does he give the reader an inkling of the great debates that have swirled around the amendment. Indeed, he informs us in one place that it is not his intent "to place the Ninth Amendment in its historical context" (i.e., to explain its original meaning; p. 24 n.40). Yet, he proceeds as if he has established the historical meaning of the amendment. In sum, this is another book that expresses unhappiness over the paths the Supreme Court has wandered down in the past half-century; unfortunately, it is also one that contributes little to the ongoing discussion.

Counting on the Latino Vote: Latinos as a New Electorate. By Louis DeSipio. Charlottesville: University Press of Virginia, 1996. 221p. $35.00.

Rodney E. Hero, *University of Colorado*

Scholarly as well as popular discussions of Latinos and politics in the United States are often poorly informed empirically and not well "situated" theoretically, resulting in a host of dubious claims and conclusions. Only recently has solid evidence that considers the several major Latino groups across the nation become available, permitting systematic analysis of their attitudes and behaviors. Louis DeSipio's book is an important one in that it draws from and begins to probe the meanings of that evidence. It places its inquiry within a sensible framework and uses data from two major national surveys, the Latino National Political Survey (LNPS, undertaken in 1989–1990) and the National Latino Immigrant Survey (NLIS, undertaken in 1988), to examine Latinos' opinion, attitudes, and behaviors, especially around voting. It supplements that data with other evidence on Latino voting for the early 1990s.

A strength of DeSipio's book is that it is comparative at several levels. It begins with an historical framework that contrasts Latinos with other "new electorates" of the twentieth century—women, African Americans, white ethnics, and youth. The concluding chapter revisits the comparative framework and not only considers recent and present patterns but also speculates on several future scenarios for Latinos. Understanding, and underscoring, the diversity of the several Latino groups, DeSipio also makes a systematic comparison of Mexican-Americans, Puerto Ricans, and Cuban-Americans across a number of questions. Furthermore, he examines and seeks to explain the behavior of several subgroups within the Latino population. These comparisons also make the work comprehensive within the domain of participation issues it considers, although understanding Latinos and U.S. politics also requires attention to a host of other issues that are beyond the purview of this book.

The analysis begins with a necessary starting point: a discussion of the myths and realities of Latinos' partisanship, ideology, and policy preferences. Among the notable findings is that Latino voting in support of Democratic candidates has been declining, although the reasons are unclear. Also, Latino ideology and policy preferences are more complex than is generally understood. For example, it may be surprising to some that Latinos are more likely to describe themselves as conservative than liberal. At the same time, however, large proportions of Latinos support the need to have government involved in addressing problems. Similarly, almost two-thirds of Latinos (across the several groups) believe that there are too many immigrants in the United States. Yet, they very much oppose measures and policies that discriminate against or otherwise treat immigrants unfairly; they have a strong civil rights orientation.

Several chapters (3 through 6) examine the book's core questions. Essentially, those are: How large and consequential is the actual and potential Latino vote? What explains Latinos' (non)voting? What policy and/or partisan consequences would major increases in the size of the Latino vote likely have? DeSipio disaggregates these questions. To consider the first, he looks at evidence on recent elections. He finds, for instance, that Latino votes appear to have had significant consequences for the 1992 presidential election outcome in several states; that Clinton carried Colorado, New Jersey, and New Mexico seems attributable in part to Latino support.

DeSipio considers the voting questions by identifying different types of nonvoters, delineating three groups: the Reticents are U.S. citizens who were registered but did not vote in 1988; the Reluctants are citizens who are not registered; and the Recruits are noncitizens eligible for naturalization. While the explanations are somewhat complex, DeSipio argues that Latino nonvoting is best understood in socioeconomic terms, that is, income, education, and age are the strongest factors associated with nonvoting, especially among the Reticents and Reluctants. Institutional factors are not discussed concerning Reticents and Reluctants, and it is unclear why. One is left with the impression that, perhaps because of the Voting Rights Act of 1975 and the Motor Voter legislation, DeSipio perceives institutional variables as no longer important. But institutional factors are emphasized in his consideration of the noncitizens, the Recruits.

Evidence from the NLIS and elsewhere suggest to DeSipio that many Latino noncitizens are frustrated in their naturalization efforts by policies and procedures of the Immigration and Naturalization Service. While the agency's policies seem simple and straightforward on the surface, he indicates several ways in which they discourage Latinos from achieving formal citizenship. He also notes that several individual-level traits are associated with seeking citizenship.

DeSipio then examines each group of nonvoters systematically, assessing their political orientations and speculating on their likely effect if they did begin to participate. The political orientations of these groups are such that higher levels of participation probably would not lead to major new policy pressures on the system. Indeed, a number of DeSipio's findings should allay "fears" about the Latino population.

On the whole, the book is deliberate and systematic in addressing the questions it poses. Several problems or questions may be raised, however. The book sometimes too easily takes its findings at face value. When it discusses Latinos and their political ideology, for example, there is insufficient attention to exploring what those findings mean. Are the terms "liberal" and "conservative" adequate to capture political orientation; do they necessarily have the same meaning to or for Latinos as others (indeed, research on non-Latinos casts doubt on what to make of ideological self-identification); should we distinguish among social, economic, and/or other dimensions of ideology? In examining policy preferences, one survey question asked whether the United States should be more involved in Central America. But with no substantive content—that is, involvement of what sort, toward what ends—it is difficult to know what to make of the responses. Several other survey questions that are drawn upon seem similarly ambiguous.

Also, in considering the alternative scenarios or possible "trajectories" of Latinos as a new electorate, more thought might have been given to the type of responses the trajectories may engender in U.S. society. There may well be a strong backlash against real, or imagined, efforts to mobilize the Reticents and Reluctants, much less the Recruits, into the political arena. There has already been evidence of such resistance, and this has occurred in the absence of major mobilization campaigns.

Such criticisms aside, overall this book provides a practical framework, good evidence, and rigorous examination. It illuminates an array of important issues and does so thoughtfully. Its findings will be a benchmark in understanding Latino voting, its effect, and its development.

Presidential War Power. By Louis Fisher. Lawrence: University Press of Kansas, 1995. 245p. $29.95 cloth, $14.95 paper.

David Gray Adler, *Idaho State University at Pocatello*

Louis Fisher, prize-winning scholar and the nation's leading authority on separation of powers, has produced a trenchant and provocative study of one of the twentieth century's most important constitutional issue, the repository of the authority to initiate war. Fisher vigorously confronts presidential claims to a unilateral war-making power and demonstrates that the Constitution assigns to Congress the sole and exclusive power to commerce and authorize war. The president is granted only the authority to repel attacks against the United States.

Fisher's spare, lean, and lucid prose illuminates the controversy that has surrounded the question of the legality of executive war making. The framers of the Constitution rejected the prevailing models of John Locke and Sir William Blackstone, which placed virtually unfettered control over foreign affairs and the war power in the hands of the executive. The Constitutional Convention broke sharply from that model and lodged in Congress the lion's share of foreign policy powers, including the power of war and peace. The "original intent" of the Constitution on this issue could hardly be clearer. The power "to declare war," as the convention debates and the early practice established, meant that all wars, large or small, declared or undeclared, had to be authorized by Congress. From the framers' perspective, the executive should not be permitted to initiate military hostilities against a foreign nation since one person could not be trusted with such awesome authority. The framers, then, embraced the theory of collective decision making, not only in domestic affairs but also in foreign affairs, a scheme that is readily apparent in the Constitution's assignment of senior status to Congress in a partnership with the president for the purpose of conducting U.S. foreign policy.

Until 1950, no president had claimed the constitutional authority to take the nation into war without prior congressional authorization. But President Harry Truman's assertion of that power, discussed in chapter 4, ushered in an era of presidential usurpation of the war power, a pattern that has been repeated, most recently, by Ronald Reagan, George Bush, and William Clinton. As Fisher demonstrates, however, this claim finds no support in the records of the Constitution, the debates in the state ratifying conventions, the pages of the Federalist Papers, or in judicial decisions. In short, there is no support for unilateral presidential war making in our constitutional architecture.

Fisher's examination of the principal legal arguments adduced by defenders of presidential war making is balanced, to be sure, but it pulls no punches. The commander-in-chief clause, often invoked by presidents, confers no war-making authority upon the president; indeed, such a reading would eviscerate the constitutional grant to Congress of exclusive authority to decide for war. Presidents and their defenders have advanced the contention that circumstances in the realm of international relations require speed and dispatch, requirements which preclude response by a lumbering body such as Congress and are conducive to the efficiency and unity of the executive. Fisher rightly rejects that argument: "Few uses of unilateral force by the president can be explained by genuine emergencies. Contrived emergencies, perhaps, but not real ones. In the period since World War II, only *one* situation justified presidential action in the absence of congressional authority: President Ford's evacuation of Americans and foreign nationals from Vietnam" (p. 186). For Fisher, if presidents believe that an emergency requires force before approval can be obtained from Congress, then it is far better for them to act and later seek retroactive authorization from Congress on the basis of an explanation of what they did and why they acted. Lincoln rightly followed that procedure in the Civil War because, as Fisher notes, "Congress is the only branch that can confer legitimacy on an emergency measure" (p. 187). Presidents Truman, Bush, and Clinton have variously invoked national defense treaties and UN Security Council resolutions as legal justification for military initiatives. But Fisher justly rejects these claims: "Treaties entered into by the President and Senate—whether for NATO or the UN Charter—cannot strip from the House of Representatives its constitutional duties over military commitments" (p. 189).

Perhaps no other provision of the Constitution has been as flagrantly violated as the war clause. Presidents since Truman have unilaterally taken the nation to war on the assumption that they may do anything they wish with the nation's armed forces. Members of Congress, largely ignorant of their constitutional responsibilities and powers in the realm of foreign affairs, and overarchingly concerned with matters that will facilitate their reelection efforts, which, they have decided, emphatically does not include foreign policy decision making, sits on its hands and acquiesces in the face of executive presidential usurpation of the war power. And the judiciary, with the exception of Federal Judge Harold H. Greene's splendid decision in the Persian Gulf War case, *Dellums* v. *Bush* (752 F. Supp. 1141 [D.D.C. 1990]), in which he declared that the president may not commence military hostilities without prior legislative authorization, has expanded, not restricted, presidential power in foreign affairs.

The constitutional crisis in foreign affairs, then, involves complicity among all three branches of government. The essence of Federalist 51, that "ambition must be made to counteract ambition," which Madison believed would maintain constitutional equilibrium, appears today to have little currency. What is to be done? Fisher rightly seeks a renewal of Madison's emphasis on congressional integrity and responsibility. He states: "Each branch must protect its own territory. Congress cannot go to the courts, hat in hand, asking judges to do what legislators are fully capable of doing: check the president. Congress should not entrust to the judiciary the duty of protecting legislative prerogatives" (p. 199). As Justice Robert H. Jackson said in the steel seizure case, "only Congress itself can prevent power from slipping through its fingers" (343 U.S. at 654). Fisher is surely correct in insisting on congressional rectitude, but is it too much to ask the same of presidents and courts?

Fisher's book constitutes a powerful contribution to the discussion of the war power. It provides connective tissue to the earlier works of Wormuth, Berger and Bickel, Koh, and Glennon and Ely, among others, and it illuminates the contemporary relevance of the framers' perspective on the decision to go to war: Congress, and not the president, should make that fateful decision.

Studying Public Policy: Policy Cycles and Policy Subsystems. By Michael Howlett and M. Ramesh. Toronto: Oxford University Press, 1995. 239p. $28.00.

David A. Rochefort, *Northeastern University*

This book, coauthored by a Canadian and an Australian political scientist, is one of the most useful general works to appear recently in the field of public policy. Its main purpose is to give an overview of the policy process using the familiar framework of the sequential policy cycle. The authors use this approach, however, not to simplify their material but to organize a complex analysis of the institutions, instruments, actors, ideologies, and organizational factors that interact in various ways at each stage of the policy process. The discussion is up-to-date, cogently assembled, and conceptual in its orientation.

Most chapters tend to resemble detailed literature reviews. The authors systematically present the main scholarly perspectives on a research question, then provide their own assessment and synthesis. Often, this includes the introduction of insightful new analytic categories or typologies. Eschewing the limitations of theorizing that is either narrowly inductive or deductive, Howlett and Ramesh progressively build an alternative model of policy development that is multicausal and geared to the statement of empirically grounded propositions. This task they characterize as one of constructing theory "in the middle range" (p. 41).

The chapter on policy instruments, an area in which Howlett and Ramesh have done substantial prior work, is especially strong. Also well presented is the process of policy formulation, including distinctions in the research on issue networks, advocacy coalitions, policy networks, and policy communities. A concluding chapter perceptively contrasts the idea of "normal" and "paradigmatic" policy change, noting the different kinds of politics and discourses associated with each.

Elsewhere, the authors properly emphasize an approach to agenda-setting analysis that is comparative across issues as well as different political regimes. In highlighting the nature and characteristics of a policy problem as crucial determinants of its political trajectory, however, they acknowledge the importance of problem definition without drawing specific insights from this growing field of investigation. Too, the authors assert incorrectly that "little empirical work has been done on agenda-setting in recent years" (p. 117).

The book's weakest chapter is the one addressing policy implementation. Here, the authors choose to focus further on instrument selection, giving but scant attention to the extensive multidisciplinary literature on the intricate process of program assembly and organizational politics at the operational level. In this way, they implicitly reinforce an outmoded notion of public management as outside the policymaking process.

Repeatedly, Howlett and Ramesh assert that readers will be unable to appreciate the application of general observations, or put together the phases of policymaking into a coherent whole, without focused policy analyses. For example, in discussing policy formulation, they explain: "The nuances ... in particular instances can be grasped only through empirical case studies" (p. 123). It is unfortunate, therefore, that *Studying Public Policy* generally lacks in-depth discussion of specific policy domains. The absence of fully developed case material is an understandable function of the thematic organization of this book. Even so, however, it should have been possible to weave in more policy details and illustrations within and across the several chapters.

Finally, the authors at times succeed better in representing parliamentary democracy than the U.S. polity. More than once, they maintain categorically that "only in a limited number of circumstances are legislatures able to influence the policy process and its outcomes" (p. 199); this contradicts other observations about the fundamental legislative differences between parliamentary and presidential systems. The treatment of intergovernmental dynamics under federalism, a subject of major significance in understanding public policymaking in the United States, is also insufficient and fails to credit the states' major contributions to policy innovation.

These problems notwithstanding, *Studying Public Policy* provides a sophisticated guide to contemporary scholarship and conceptualization on diverse aspects of the policymaking endeavor. Howlett and Ramesh succeed admirably in portraying the complexity and contingent nature of the process, as well as the rich intellectual tradition that is giving birth to policy science as its own unique discipline. The high level of discussion in this book will put it out of the reach of most students at the introductory level, but more advanced students and faculty seeking a broadly gauged survey of the public policy field should find this work indispensable.

Science at the Bar: Law, Science, and Technology in America. By Sheila Jasanoff. Cambridge, MA: Harvard University Press, 1995. 285p. $29.95.

Andrea Bonnicksen, *Northern Illinois University*

A book-length narrative that explores in an analytically provocative way the interrelations among science, technology, and the law is welcome. Sheila Jasanoff, chair of the Department of Science and Technology at Cornell University, has produced such an exploration in *Science at the Bar*. Although numerous authors have published case studies exploring the legal and ethical implications of scientific innovations, few have invited theoretical thinking about how the institutions of science and the law converge and diverge in a democracy.

Not untypically, scholars regard science and technology as independent variables and legal decisions as dependent variables. This model places judges and courts in a reactive role. Unwilling to accept this model, Jasanoff explores the way legal institutions enrich and contribute to the public's perceptions of science and technology. According to Jasanoff, the law "constructs the very environment in which science and technology come to have meaning, utility, and force" (p. 16). Courts are proactive in the way they deconstruct expert authority, educate the citizenry about science and technology, and make effective decisions that have an influence on daily lives.

Science at the Bar reviews key scientific innovations and legal decisions that have emerged over the past 70 or so years. Jasanoff discusses expert testimony in court (chapter 3), scientific decisions by administrative agencies (chapter 4), scientific research (chapter 5), toxic torts (chapter 6), genetic engineering (chapter 7), reproductive technologies (chapter 8), and end-of-life technologies (chapter 9). The courts emerge as useful entities that interpret science and technology for public use. Knowledge, asserts Jasanoff, comes from the interaction between science and technology, on the one hand, and adjudication, on the other (p. 137). For example, she discusses how courts "deconstruct" science by exposing to cross-examination the testimony of experts and unveiling the biases and interpretations of witnesses. She also traces cases in which courts effectively "force agencies to make themselves understandable to the lay public" (p. 92). Courts do more than just "adopt the prevailing scientific opinion" (p. 207); judges are interpreters who stand " 'at the frontiers of scientific knowledge' " (p. 210).

Concerned that judges lack scientific savvy, observers periodically propose that judges should read manuals or undergo science training in order to think more like scientists or that the judicial system should be bypassed in favor of alternatives to litigation. Jasanoff resists such suggestions for practical reasons (the manuals would become dated) and for ideological reasons (courts perform a service by making scientific knowledge decipherable to citizens). She does entertain the wisdom of scientific advisory boards and encourages scholars to "deepen the agenda of legal research" by supposing that courts help make science democratic. For example, she suggests examining the dicta of legal opinions to identify features of a judicial ideology and investigating why citizens look to the law to resolve scientific conflicts.

Although this book piques the scholar's interest in studying the relation between law and science, in some ways its effect falls short of its potential. First, the book would benefit from a fuller explication of why it needed to be written. The author's purpose is to look "more closely at what actually takes place in legal disputes involving science and technology" (p. 2), but it is not immediately clear why that close look is important. The author writes of a "groundswell of discontent" in which the "legal system is again being urged to leave the resolution of scientific disputes to scientists" (p. 1), but she does not provide evidence of serious discontent.

Second, the concepts of law, science, and technology emerge as monolithic entities (e.g., "the law's view") that are distractingly elusive. It might be more manageable to deal with the judges, scientists, and litigants, as the author herself suggests by encouraging research into how individuals perceive judicial involvement in scientific decisions. Bringing the abstract concept of the "law's response" to the level of discrete units of analysis would help ground and illustrate the book's general themes.

Third, the book's ideas are important enough that the reader yearns for a clearer statement of its many subthemes. Questions abound in this book, yet not necessarily to good effect because the author poses questions in each chapter that lack rigor and are too often left unanswered. A welcome addition would be position statements in each chapter that can be supported or rejected, depending on the evidence presented. To illustrate, the author states at the start of the chapter on research that "our task here is to determine whether there are areas of scientific inquiry that courts systematically choose not to enter or supervise, and what reasoning supports their decisions not to intervene" (p. 93). Turning that idea to clear conclusions and testable propositions would add rigor to the analysis.

Fourth, the historical review of cases is sometimes overly ambitious. For example, the rationale for selecting cases in the chapters on reproductive technologies and end-of-life decisions was unclear. Rather than moving quickly over a series of cases, the author more effectively might have used one or two cases to illustrate the principles she wanted to document and then could have developed hypotheses and reasoned conclusions. The amount of information contained in each chapter made it difficult to absorb. Adding a more cogent conclusion to each and giving a clearer idea of the chapter's operating presumption would do much to help the reader determine what is more and less important in the review of case law.

Innovations in science and technology often lead to compelling and highly publicized judicial controversies. Critics charge that the courts exert a nondemocratic influence on decision making in a technological society, yet, citizens repeatedly turn to the courts for solutions. *Science at the Bar* takes an alternative position: It credits the courts with exerting a democratic influence on the way citizens grapple with scientifically induced conundrums. Because litigants continue to look to the courts for answers, Jasanoff's claim that judicial action does not abrogate democratic responsibility is reassuring. It is not uncommon for judges in cases dealing with biomedical innovations to lament that the absence of legislative guidance forces them to become courts of first impression. This book encourages scholars to study how the judiciary as pioneer is not necessarily a problem.

The System: The American Way of Politics at the Breaking Point. By Haynes Johnson and David S. Broder. Boston: Little Brown and Company, 1996. 668p. $25.95.

Boomerang: Clinton's Health Security Effort and the Turn against Government in U.S. Politics. By Theda Skocpol. New York: W.W. Norton, 1996. 230p. $27.50.

Lawrence R. Jacobs, *University of Minnesota*

Social science research is typically driven either by real-world problems and developments or by theoretical and methodological issues raised by previous research. *The System* and *Boomerang* both address a real-world policy development that dominated the news in 1993 and 1994—President Clinton's failed campaign to pass health care reform.

Soon after Democrats pulled the plug on health reform in September 1994, pundits pinned the defeat on personalistic factors. The caricatures are by now familiar: Senator Daniel Patrick Moynihan and Representative Jim Cooper as the Democrats' Benedict Arnolds, Ira Magaziner as Rasputin, and Hillary Rodham Clinton as the arrogant know-it-all. Many blamed Clinton's personal leadership traits and pointed to his tactical blunders—his establishment of a secret task force that locked out allies and fed the worst fears of moderates, his decision to delay his legislative proposal until the opposition was mobilized and his own popularity was ebbing, and his inept public relations campaign after announcing the administration's proposal in September 1993.

Clinton's failed health reform is a critical case for social scientists because it distills some of the most demonstrable aspects of contemporary national politics in a way that pundits have largely missed. Two levels of explanation are missing that are standard fare in the political science repertoire, both of which avoid the singularly personalistic accounts marketed by pundits. First, a situational analysis would indicate that the president's probability of success was always low because of such unfavorable conditions as a weak electoral mandate. Second, institutional explanations point to a series of enduring processes that obstruct major new policy initiatives: politicization and centralization within the executive branch; individualization within Congress; proliferation of sophisticated pressure groups; and journalists' fixation with conflict and insider strategy.

The books by Skocpol and Johnson and Broder form the front wave of what is becoming a tidal wave of work on Clinton's failed health reform. Both books persuasively dispel the shallow accounts offered by pundits and begin the process of developing situational and institutional analyses.

Johnson and Broder have woven a tapestry of masterfully drawn portrayals of key participants and discussions. Their book is largely organized chronologically, charting the formulation of Clinton's plan and the twists and turns of the subsequent debate over the president's proposal. Their account is based on a veritable mountain of material—official memoranda, journalistic accounts, and interviews with about 150 individuals, some of whom talked with the authors a number of times during the course of the episode. The result is a kaleidoscope of constantly shifting perspectives: Clinton administration officials, Democratic and Republican members of Congress, interest groups, community health care providers, and focus group participants all offer their recollections and interpretations.

Johnson and Broder have produced a comprehensive and highly detailed description of the political maelstrom that descended on Clinton's health reform initiative. They have written what will quickly become the source book for the politics of this episode, one that a generation of political scientists will use to write case studies or prepare class lectures. (The book serves up a sumptuous assortment of anecdotes that illustrate a range of political behavior, from strategic gamesmanship to bare-knuckled retribution for disloyalty.)

The System, however, suffers from several limitations. First, all too often it slips into post-hoc characterizations, reading current thinking back into the behavior of individuals in 1993 and 1994; lost are the strategic openness of the moment and the alternative directions that were available. For instance, following Clinton's speech of September 1993, the book shifts to a focus group and proceeds selectively to emphasize doubts, especially those concerns that would become more pronounced over the next year. This account of public opinion obliterates the multiple and competing considerations that Americans harbored in 1993 (and since) and that could have formed an alternative evaluation of health reform. (Whether the authors inappropriately—by implication—generalize focus group discussions to national public opinion is another question.)

In addition, *The System* lapses into mischaracterizations of U.S. and comparative health care policy. For instance, it states that single-payer systems were adopted in "Canada, Great Britain and most European countries" (p. 71). Lumping the disparate arrangements in these countries into one category mystifies rather than clarifies. Furthermore, using the single-payer label, which is typically associated with the Canadian system of negotiated provincial reimbursement arrangements and private health care delivery, to characterize the British system of national governmental financing and delivery of health services is a positive disservice to public education and debate.

Finally, the book's finely detailed description does not add up to a coherent and parsimonious explanation. Devoting 668 pages largely to describing who said what to whom is far from an analysis that systematically identifies the critical influences which doomed Clinton's effort. *The System* would be more aptly titled "The Actors" because of its preoccupation with the key actors and their thoughts and actions.

Despite the clear connotation of the title, the book fails to provide a sustained discussion of the organizing principles or tendencies that arise out of political structures and give coherence to what are otherwise separate, isolated actions by individuals. An example is the Johnson and Broder account of Clinton's decision to centralize his health reform task force in the White House and to put Hillary Clinton and Ira Magaziner in charge. The authors reduce this decision to the president's predisposition to depart from the conventional process of experts within the departments directing policy formulation in the executive branch. Aside from the questionable accuracy of the authors' portrayal of the "conventional" process for major presidential initiatives, this discussion is mistakenly pitched at the individual level of analysis. In fact, the entire thrust of the institutional development of the modern presidency has been toward both centralizing critical decisions within the White House and politicizing the executive branch by knighting individuals to run departments and major policy initiatives because of their political loyalty to the president rather than their neutral policy competence. Clinton may have nudged the trend a bit farther by (among other things) appointing his wife to run his health reform initiative, but his actions are fully consistent with an institutional tendency—enmeshed in a set of linked incentive structures—that is evident in previous presidencies and in other domestic and foreign policy areas. (When was the last time major foreign policy decisions were directed by the Department of State rather than the White House?)

Although scholars and teachers will find plenty in *The System* to mine for their own research and lectures, the book is not well suited for either undergraduate or graduate courses in which interpretative rather than simply descriptive material is most useful and appropriate. The book is but the first step toward a systematic explanation for the 1993–1994 health reform episode that melds personalistic, situational, and institutional analyses.

Whereas Johnson and Broder take a microscope to Clinton's health reform crusade, Skocpol uses a telescope. She plots the rough topography of the episode, relying on secondary sources—with a selection of memoranda and interviews.

Skocpol suggests that "defeat" can mean many things. In Clinton's case, it meant more than just adding another casualty to a century-old battle to legislate comprehensive health reform; the failure to pass legislation also catapulted Republicans to ascendence in Congress and launched the outright assault on established social policy programs. Like a boomerang, Clinton's failure to achieve health reform turned back on the president and Democrats—with the aid of "right-wing government haters"—and produced legislative and political consequences that were exactly the opposite of what Democrats intended. Skocpol concludes that Clinton's failed initiative was a "probable turning point in the history of twentieth-century U.S. social policy," "reversing decades-long momentum toward public regulations and social protections" (p. 171)

Skocpol has largely organized her book into thematic chapters that identify what she sees as the causes and consequences of failed health reform. These chapters map the mobilization of opposition by conservatives, divisions among Democrats, and the administration's public relations campaign. (Skocpol criticizes the Clintons' decision to eschew substantive messages in favor of a broad "security for all" theme because it gave reform opponents free reign to mischaracterize the mechanics of the president's plan.) In the conclusion and elsewhere, the book unfurls a progressive policy agenda that at once posits the expectation that U.S. leaders are responsible for using government to address pressing social problems and then slams members of Congress, journalists, and the country's governmental institutions in general for failing to deliver. Her recommendation is that the United States should expand and not contract its welfare state; Clinton is criticized as an overzealous deficit hawk and an unwilling "Democrat in the New Deal tradition . . . [who would combine] new Federal regulations with generous subsidies" (p. 182).

Boomerang shifts the discussion of Clinton's health reform from description to institutional and historical analysis. Skocpol peppers the book with comparative and historical references that begin the process of enriching and broadening the discussion of this episode. The book discusses, for example, the European corporatist models and their inappropriateness in the United States in order to dispel alternatives to the Clinton task force; Skocpol refers to the role of courts in reviewing social legislation during the Progressive era and the New Deal to situate the current clout of the Congressional Budget Office.

Skocpol's most recent book is not comparable to her previous works. Its format, language, and writing style are intended for a wider audience, and its purpose is to intervene in contemporary political debate. This book is neither concerned with theory building nor with large-scale data collection.

Even when judged on its own terms, however, the book raises several questions. Although it finds fault with the political decisions of the Clinton White House, the book devotes large sections to explaining, justifying, and sometimes defending the administration's handling of health reform. This is probably a useful corrective to the pervasive personalistic accounts and is a productive challenge to important components of the reigning conventional wisdom about what went wrong.

But this approach also comes with a cost. Reasonable and thoughtful criticism and opposition to the Clinton proposal existed and are given short shrift in this book. In addition, *Boomerang* all too readily and uncritically adopts the assumptions, explanations, and policy mindsets of the administration. For instance, it reports that the Clinton proposal to achieve universal coverage and cost control was "no liberal scheme" but "a carefully constructed compromise between previously available liberal proposals and more conservative, market-oriented ideas about health care reform" (p. 17). There is no question that this is what the Clintons intended, but whether this mindset was accurate is an entirely different matter. In fact, the Clinton proposal was a more radical expansion of government than the prototype liberal single-payer scheme: The latter proposed to extend government involvement into the financing of health care, while the former aimed to thrust the government into both the financing *and* delivery of services.

Moreover, declaring Clinton's failed health reform campaign a "turning point" both overstates its effect and understates its precedents. The Republican romp in 1994 was historically distinctive, but it is premature to declare a qualitatively new era in which the GOP will gain a lasting hold on power and dismantle policy affecting mainstream America. Clinton's failure represents not a dramatic new departure but, rather, one more point on a continuum of developments during the past twenty years that have restructured (and not dismantled) the U.S. occupational welfare state. After all, the unraveling of the New Deal coalition and the shift from the growth politics of the 1970s has produced a string of presidents—from Carter through Reagan and Bush—who have questioned progressive government policy. The direction of policy restructuration has been directly related to the power of sectional interests and the character of existing social policy. Finally, Skocpol's call for a "renewed social vision" of progressive politics seems to rely on volition and individual will rather than on future situational or, more tellingly, structural developments.

With characteristic vigor, Skocpol has staked out a strong counterpoint to the prevailing orthodoxy about Clinton's failed reform and the pariah status of increased taxes and government. Readers with interest in U.S. politics and in Clinton's health care reform campaign may disagree with *Boomerang*, but they should not ignore it.

Divided by Color: Racial Politics and Democratic Ideals. By Donald R. Kinder and Lynn M. Sanders. Chicago: University of Chicago Press, 1996. 393p. $27.50.

William Claggett, *Florida State University*

Divided by Color is an important book. It examines why white and black Americans hold the opinions they do on policies designed to enforce or produce racial equality. It is also about the possible consequences of these opinions for the conduct of U.S. election campaigns. The book is methodologically sound, based on high-quality survey data (mostly the 1986 NES, supplemented by other recent NES and GSS data), and readable. As a result, its conclusions should and will be given serious consideration. It is also a fundamentally pessimistic book since it concludes that racial resentments still drive the opinions of many whites and that elites continue to be tempted to appeal to these resentments for electoral advantage.

The book opens with a review of the current deep racial divide over six policies designed to produce racial equality: whether the government should ensure the fair treatment of blacks in employment; whether the federal government should see to school integration; whether the federal government should increase, maintain, or decrease spending on programs that assist blacks; whether the federal government should make every effort to improve the economic and social position of blacks; and whether blacks should be given preferential treatment in hiring and college admissions. The racial divide on these policies is wide, far larger than the differences between other demographic groups on other policies, and is not due to socioeconomic differences between whites and blacks. It is these racial differences and the variation within each racial group that Kinder and Sanders wish to explain.

The authors look for an explanation in terms of three sets of individual-level attributes and one element of the political environment. The three attributes they examine are self-interest, political principles, and racial resentments. The latter, or what earlier researchers termed symbolic racism, end up being the key individual-level explanatory variable shaping the opinion of whites. Kinder and Sanders argue that racial prejudice has changed in the United States. A biological racism has given way to a racism based on a conjunction of prejudice and values, or what Kinder and Sanders call principles, especially values reflecting a secularized version of the Protestant ethic. These resentments are measured by a battery of six questions to determine whether whites believe that blacks are trying hard enough to overcome their problems and whether they are receiving undeserved rewards. By controlling for self-interest, principles, and a host of demographic factors, the authors demonstrate that white opinion on these policies is primarily due to racial resentments. Moreover, white opinion on issues outside the domain of race are also influenced by these racial resentments.

Two of three political principles investigated by Kinder and Sanders also shape the opinions of whites and blacks. For whites, support for equality and limited government both have a reasonably strong influence on their racial policy opinions. Equality also has a strong effect on the opinions of blacks, while the influence of limited government is more restrained. Economic individualism, reflecting a person's belief about whether individual striving is rewarded, is unrelated to the racial policy opinions of both whites and blacks. With regard to personal or family self-interest, the findings are now unsurprising; the opinions of neither whites nor blacks are shaped by self-interest. Furthermore, whites' perception of racial threats to their interests is unrelated to objective conditions but strongly influenced by their racial resentments. Perception of group interest does matter a little, however, for the racial opinions of both whites and blacks.

But the racial opinions of Americans are more than just the product of an individual's characteristics. According to Kinder and Sanders, how elites frame issues during political campaigns also influences mass opinion by priming certain considerations rather than others. They first demonstrate in a series of question-wording experiments that slight alterations in wording can increase or decrease the explanatory role of racial resentments and principles. They also demonstrate that overall support for racial policies can be increased if the beneficiaries are minorities or poor people rather than blacks. They then try to connect these question-wording experiments to actual framing of racial issues in current political campaigns by tracing out the changing role of race in U.S. politics since the mid-1950s. The result of this evolution is that Democratic electoral success is dependent on reducing the saliency of race in elections—on a politics of benign neglect—while Republican success is dependent on drawing support from racial conservatives without appearing racist, which is achieved through the use of racial codewords. Kinder and Sanders then examine the 1988 presidential election to illustrate these differing temptations of race in U.S. election campaigns.

Two shortcomings of the book should be mentioned. The first concerns the causal nexus among prejudice, principles, and racial resentments/symbolic racism. If racial resentment is the combination of prejudice and principles, as the authors suggest (pp. 292–3), then we need to measure all three concepts and determine not only their direct effects on racial opinions, which is what the single-equation models that

Kinder and Sanders employ in all but one instance do, but also their total effects. This is, of course, really a theory question about the causal interrelationships between principles and racial resentments. Whatever these causal pathways may be, they probably are more complex than those reflected in single-equation models.

The second shortcoming is that the book begins by looking at the racial divide in public opinion, a divide which to the authors was the most "arresting feature of public opinion on race" (p. 287), but actually has very little to say about why that divide is so wide. The basic problem is that almost all the analyses are carried out within racial groups. As a result, we learn a great deal about the factors responsible for the variation in racial policies among whites and blacks separately, but factors that do or do not account for within-group variation may play very different roles in explaining variation between groups.

Despite these limitations, *Divided by Color* is a significant piece of scholarship. It is destined to have a central place in the ongoing debate over the role of race in U.S. politics and public opinion.

David Duke and the Politics of Race in the South. Edited by John C. Kuzenski, Charles S. Bullock III, and Ronald Keith Gaddie. Nashville, TN: Vanderbilt University Press, 1995. 161p. $19.95.

J. Morgan Kousser, *California Institute of Technology*

Do we really need a book of short, rather disconnected essays by political scientists about David Duke, the Louisiana Nazi-Klansman-Republican whose bubble career temporarily set back, rather than forwarded, the racist cause in which he so fervently believes? In 1992, a group of political scientists, historians, journalists, pollsters, and political activists published *The Emergence of David Duke*, edited by political scientist Douglas D. Rose. It was a set of essays, not reviewed in this journal, that provided first-hand, often gripping accounts of Duke as well as original poll results and analyses of precinct-level aggregate data that illuminated the traits of his supporters. Produced by members of a coalition organized to expose the truth about the well-spoken, stylishly accoutred, surgically enhanced Duke, the Rose volume made up in liveliness what it sometimes lacked in objectivity.

Several of the papers in *David Duke and the Politics of Race in the South*, which for convenience I will refer to as "Kuzenski," respond to essays in the Rose book or rely on facts contained in it. For example, Kuzenski assumes that readers are generally familiar with biographical details about Duke, that they understand the significance and at least some of the nature of the controversies about the identities of Duke's supporters, and that they know the general outlines of the development of recent southern politics. Unlike self-contained books that can be readily assigned in class or easily read for self-improvement, Kuzenski requires a good deal of previous knowledge—which is perhaps best gained by first reading the Rose book.

Averaging 17.2 pages and 3.7 tables, figures, or maps each, most of the nine workmanlike papers in Kuzenski seem formulaic: State an hypothesis, run some regressions, discuss the results. They rarely specify competing models or respond explicitly to contradictory findings or methods in the other essays in the book, which is surprising, since there is considerable disagreement among the authors. For instance, in chapter 3, John K. Wildgen strongly criticizes the use of "ecological regression" on aggregate data (while employing a form of it himself), while in chapters 4, 5, and 8, Stephen J. Caldas, John C. Kilburn, Michael W. Giles, Melanie Buckner, Ronald F. King, Douglas D. Rose, and Matthew Crozat show no hesitation in running regressions on parish- or precinct-level data. In chapter 7, T. Wayne Parent briefly analyzes the literature and concludes that conservative racial attitudes, not class positions or stances on other social issues, best account for the rise of the southern Republicans, while in chapter 8, King, Rose, and Crozat find almost no Republicans voting for Duke, the quintessence of racial backlash, in the 1991 nonpartisan gubernatorial primary and the 1992 Republican presidential primary. Wildgen treats Duke support as a mere symbolic protest against "political correctness," irrelevant to what he believes to be the principled, nonracist stands of the Republican Party, while Giles and Buchner in chapter 5 view Duke loyalists as responding to the racial threat of nearby African Americans, creating a pattern of votes similar to that of George Bush in the 1992 presidential campaign (pp. 43, 60, 94–98). Kuzenski's own essay in chapter 1 asserts that the peculiar modern Louisiana institution of the statewide nonpartisan primary significantly helped Duke rise, while chapter 9, by Euel Elliott and Gregory S. Thielemann, argues that Duke lost in the Republican presidential primary in Louisiana in 1992 largely because a man with a very similar message, more money, and a less compromising past, Pat Buchanan, preempted Duke's constituency.

This last example points to the fundamental question about Duke, which the authors of these short, largely ahistorical essays are not in a very good position to ask: What is David Duke's significance in the politics of the South and the nation? Was he just a slick kook, a magnet for a relatively small and nontransferable group of racial extremists? Or did he play a crucial role in leading discontented white working-class Democrats into the Republican coalition? How coherent was his ideology, and how different was it from that of "mainstream" southern Republicans? Does the class nature of his appeal, compared to that of Huey Long and Father Charles Coughlin in the 1930s at home or of Adolph Hitler and Benito Mussolini abroad or of Jean-Marie Le Pen in France today, indicate that a U.S. fascism is inevitable or impossible? What effect did he have on public policy, not only as an insignificant and rather uninterested freshman member of the Louisiana legislature but also as a charismatic figure who demonstrated to politicians everywhere in the United States the powerful appeal of a cluster of race-tinged issues? Was this influence, in the long run, more or less important than the fact that Duke's continued extremism—he sold anti-Semitic literature out of his legislative office in the early 1990s—allowed moderates to couple more respectable proponents of racially conservative ideas with the man the moderates called David "Dukkke"?

Several of the essays in Kuzenski illuminate topics related to Duke and are quite well done. Two stand out: the paper by the three editors showing that Duke stimulated African-American turnout approximately as much as the candidacies of major black politicians in other southern states had done, and the King, Rose, and Crozat chapter on Duke's failure to become the first preference of the vast majority of Republican voters. But the static, fragmented approach of the book is not, as the editors claim in their Introduction, "the best way to understand completely the phenomenon at hand . . . a logical exposition on the political rise and fall of the Grand Wizard of the Ku Klux Klan" (p. xi). Duke, whose most recent obituary was written after his failure to qualify for the U.S. Senate runoff in 1996, awaits a comprehensive, cohesive treatment by a biographer or historian. As for Duke's place in "the politics of race in the South," in the words of the

Kuzenski subtitle, it is a subject that deserves a larger context and much more depth than this short book gives it.

The High Priests of American Politics: The Role of Lawyers in American Political Institutions. By Mark C. Miller. Knoxville: University of Tennessee Press, 1995. 228p. $30.00.

Cornell W. Clayton, *Washington State University*

Does it make any difference that nearly half the U.S. Congress and large percentages of other elected officeholders are lawyers? The "conventional wisdom," gleaned from behavioral analyses of roll-call voting patterns, says that it does not. Mark Miller, in *The High Priests of American Politics*, presents a persuasive case for reevaluating that view. Using a neoinstitutional analysis and data gathered from interviews with members and staff of the U.S. House of Representatives and the Ohio, Massachusetts, Maryland, and North Carolina legislatures, Miller sets out to explain how the attitudes of lawyers affect the very structure of public policymaking in the United States and yield a government "preoccupied with the procedurally oriented, incrementalist myth of rights, instead of with broader substantive questions of public policy" (p. 174).

The conventional view Miller rebuts is rooted in Eulau and Sprague's 1964 study, *Lawyers in Politics*, which found that lawyer and nonlawyer legislators displayed little difference in roll-call voting. By focusing narrowly on voting, Miller contends, "Eulau and Sprague and other behavioralist studies have missed . . . the very real effects that lawyer-politicians have on the internal political culture of [Congress], and on the practices of the institution" (p. 170). The relevant question for political scientists interested in the legislative process is not whether lawyer legislators vote differently, but whether the legislative process itself is the product of the liberal legal ideology embraced by the legal profession.

After a brief introductory chapter explaining new institutional analyses, Miller turns in chapter 2 to a discussion of how the training and professional socialization of lawyers produce a uniform world view, an ideology of legal liberalism that stays with the individual throughout his or her political life. That world view includes a "positivist rationality" and a belief in the "myth of rights" and "liberal procedural democracy." These latter tenets of the legal ideology, in particular, lead to a cramped, individualistic, and incrementalist view of democracy that crowds out considerations of broader social good that would otherwise lead to radical social reform (pp. 23–7).

Having discussed what it means to "think like a lawyer," Miller examines in chapter 3 the various roles lawyers play in the public policymaking process (as judges, legislators, lobbyists, chief executives, heads of administrative agencies, academics, and private litigators) and how they numerically dominate the nation's elective institutions. In 1988, 45% of Congress came from the legal profession compared with just 25% of the national legislature in Canada, 16% in Great Britain, 10% in Norway, and only 5% in France and Germany (p. 60).

Miller's major contribution, however, comes in the middle four chapters of the book. In chapters 5 and 6 he shows how the attitudes and career paths of lawyer legislators are different from those of nonlawyers. Lawyers are more likely to be elected early and stay in office longer, to be more ambitious and run for higher office, to rise to positions of leadership in the legislature, and to view their role in terms of a "trustee" rather than a "delegate" or a "politico." They also hold different attitudes about the courts compared to nonlawyers. They view the courts in more positive terms, are less likely to see judicial decisions as politically biased, and are likely to pay more attention to court decisions. In addition, they are less likely to think there should be routine legislative responses to adverse court decisions or to punish the courts through such measures as jurisdiction-stripping legislation.

In chapters 7 and 8, Miller discusses how these behavioral and attitudinal differences affect both the substance and process of legislation. He begins by presenting a typology of lawyer legislators: "nominal legislators" (who continue to practice law while serving as part-time legislators), "nominal lawyers" (full-time legislators who practiced law less than five years prior to their election), and "real lawyers" (full-time legislators who practiced law for more than five years prior to their election). Legislative policymaking, Miller argues, is influenced by both the quantity and quality (or type) of lawyer legislators in a given body. In chapter 7 he discusses how court reform efforts in Massachusetts during the early 1990s were thwarted by "nominal legislators," who were more likely than other lawyers and nonlawyers to see court reform as an attack on their personal financial and political interests. Likewise, in chapter 8 he traces differences in the operating cultures of three different congressional committees to the number and type of lawyers on them.

In a too short final chapter, Miller returns to a macroinstitutional analysis to suggest how both the substance and process of congressional legislation is affected by the characteristics of its dominant lawyer members. He cites several recent pieces of social reform legislation (the Tax Reform Act of 1986, the Immigration Control and Reform Act of 1986, the Americans with Disabilities Act of 1990, and the Family and Medical Leave Act of 1993) as illustrative of the rights-oriented, incrementalist approach favored by the lawyers, an approach which precludes real reform by "Narrowing the way viable political alternatives are defined" (p. 167).

This is a valuable study, both for the wealth of original data it provides and for its reconceptualization of legislative behavior. Nevertheless, the book has weaknesses. To begin with, it promises more than it delivers. The subtitle and the first three chapters lay claim to explaining how the "lawyers' culture" affects U.S. political institutions and the making of public policy. The substantive chapters of the book, however, are limited to explaining how lawyer legislators differ from nonlawyers. Forgotten in the last two-thirds of the volume is the role of lawyers as judges, chief executives, administrators, and litigators. This is unfortunate, both because an integrative study of the various roles played by lawyers in the governmental process is sorely needed and because Miller is led to make generalizations about the effect of legal culture on public policymaking that may be true of the legislative arena but not elsewhere. For example, while the "myth of rights" and liberal proceeduralism may lead to incrementalism in Congress, others have argued that these factors have the opposite effect in the judicial and administrative arenas (Glendon, Sunstein, Shapiro), where rights-based policymaking becomes an all-or-nothing process that stymies gradual consensus building and incremental policy solutions.

Miller also creates something of a strawman by attacking voting behavior studies, including the work of Eulau and Sprague. The latter, for instance, explained the voting similarities between lawyers and nonlawyers by pointing to the convergence of the professions of law and politics in the United States. This explanation is not necessarily at odds with Miller's own argument about legal culture and its effect on the legislative process. At the very least, Eulau and Sprague's findings are not inconsistent with Miller's thesis and actually could be used to support it. This considerably softens Miller's aversion to behavioral or legislative output studies. He is right to suggest that output studies *alone* will not tell us whether the preponderance of lawyers in legislative institutions affects the substance of policy outputs. But it is wrong to suggest that output studies are unnecessary or that we should "separate the study of an institution's actions from the study of its practices" (p. 173). The truth, and what Miller's study demonstrates, is that we must understand both *what* institutions do and how what they do is often a consequence of *why* they do it.

This brings me to a final point: To rebut the "conventional view" that studying lawyers as legislators is unimportant, Miller must do at least two things. First, he must convince us that lawyers and nonlawyers hold different attitudes; second, he must show us how these attitudes translate into substantive differences in policy outputs. This study is at its best when presenting evidence for the first, but it is less convincing in dealing with the second. The only two substantive case studies that Miller presents focus on legislative efforts to reform courts in Massachusetts and congressional responses to Supreme Court decisions invalidating flag burning and dial-a-porn legislation. These case studies, although interesting, examine only a limited area of legislative-judicial relations, precisely where one might expect legal training and experience to make a difference. They tell us little about other vast areas of social policymaking and how lawyer attitudes in legislatures may affect them. Nor does the brief discussion of recent reform measures in the final chapter help. Miller simply asserts, rather than demonstrates how, these "incrementalist measures" were the result of congressional acceptance of the lawyers' liberal legal culture (How do we know they are not the result of interest group pluralism?). Indeed, one can only conclude that this last chapter is meant to suggest possible case studies for future research.

Despite these weaknesses, this is an excellent book that should be read by students of the courts and lawyering as well as by students of Congress and the legislative process. It makes a critical contribution to our theoretical understanding of the way legal culture affects democratic processes in the United States and provides important direction for future empirical studies on the subject.

The Local State: Public Money and American Cities. By Eric H. Monkkonen. Stanford, CA: Stanford University Press, 1995. 191p. $39.50.

Michael A. Pagano, *Miami University*

Cities, it is argued in much of the urban literature of the past decade, have become entrepreneurial, innovative, adaptive, and flexible. Cities have learned how to maximize federal and state funds, to diversify their revenue portfolios, to adjust to the bond regulations of the 1986 Tax Reform Act, to collaborate and cooperate with other levels of government and neighboring jurisdictions; in a word, they have learned how to survive in an era of slow economic growth and popular resistance to tax increases. In particular, cities have been adept at finding new revenue sources, especially the kind that do not require general taxes to be levied. Fees, charges, and other forms of payments (e.g., leases) from individual consumers of public or quasi-public services for both operating expenses and as revenue streams for bond retirement have become commonplace. Cities make conscious decisions about the mobilization of public capital within legal and constitutional (both state and federal) constraints.

Preoccupation with contemporary city behaviors and fiscal conditions drives our policy discussions. The decline of urban America, perils of inner cities, brownfields and greenfields, urban viability and vitality, all these and other policy discussions refer to the modern, or postmodern, city. Yet, as Eric H. Monkkonen demonstrates in *The Local State*, much can be learned from nineteenth-century city behavior, especially in the area of city-issued debt and city investment in private activities. Monkkonen notes that Judge Dillon's opinion in 1868 may have reined in profligate municipal spending, but the author's careful construction of a database on debt issues in Illinois leads him to conclude that cities adapted "powerful means for dealing with private sources of capital, helping build a limited, but nonetheless powerful, local state" (p. 17). In other words, cities learned how to provide development incentives and capital to the private sector while the state tried to limit municipal powers.

In chapter 2, Monkkonen traces municipal bond issuances and default rates from 1850 to 1936, and in the next chapter he analyzes the popular and state response to municipal defaults in one state, Illinois. The state's 1847–1848 constitution ended the practice of financial support for railroads and other enterprises, but cities were not likewise restrained. Cities in the nineteenth century issued millions of dollars in debt for the express purpose of providing aid to private enterprises, especially to railroads, the most promising tool of economic development. Often, Monkkonen finds, residents of cities were convinced that their economic viability hinged on attracting a railroad, requiring cities to borrow in the bond market or raise taxes and invest in private railroads. And, in those instances in which cities could not afford the debt payment schedule or when the railroad went bankrupt, cities often defaulted. The crescendo of complaints about municipal tax subsidies to private railroads led to the 1870 Illinois constitution, which, like the earlier restriction on state investment in private enterprises, restricted municipal debt issuances for railroads.

Chapter 4 traces the historical debt of seven Illinois counties and presents the state and federal court decisions on the legal requirements over nonpayments by municipalities. In the late 1800s, a majority of state court decisions (44 of 59, according to the author) found in favor of the cities, meaning they were not obligated to pay creditors: "The general outcome of all cases where debt repayment was at issue was that municipalities did not have to pay" (p. 69). Many of the city defaults were for debt accrued prior to the 1870 prohibition on aid to railroads, or aid to private enterprises, or for debt issued in which the resulting tax increase violated the state's tax cap on municipalities.

Monkkonen proceeds in chapter 5 to trace case histories of "spectacular defaults" in four cities (selected not to be representative, but because of their "extensive documentation" [p. 79]): Duluth, Minnesota; Watertown, Wisconsin; Memphis, Tennessee; and Detroit, Michigan. He ably demonstrates that political choices were made in debt issuances, choices that reflected philosophical differences over the scope of city government, the economic future of the city, and the financial risk a city was willing to assume. He deduces from these four cases that city fiscal crises can be classified in two basic categories: "those that occur as a consequence of conscious local decisions (Duluth, Watertown, and Memphis) and those imposed by external circumstances (Detroit)" (p. 100). The case for categorizing these cities as such should be a call to the scholarly community to extend the analysis to other cities. Other possible categories of fiscal crisis might be found from a larger comparative city study.

In the final chapter, Monkkonen reflects on the practice of limiting debt and taxes for the promotion of railroads and other private activities. He argues that Illinois voters and delegates to the 1869 constitutional convention did not want cities to end their promotion of economic development, but at the same time they voted to curtail local subsidies: "Illinois voters approved limiting their own local governments' fiscal activities, even as they encouraged their local governments' pursuits of local development" (p. 123).

Monkkonen's meticulous quantitative and qualitative analyses of nineteenth-century debt issuances and state-imposed limitations on Illinois cities will sound familiar to urban analysts today. Many contemporary studies contend that cities' autonomy has been progressively eroded over the past few decades by federal statutory restrictions on tax-exempt debt issuance; the Supreme Court's decision that tax-exempt debt is not constitutionally protected (*South Carolina* v. *Baker*, 1988); and limitations imposed by states (and often the voters) on debt, taxes, revenues, and expenditures, among other federal and state constraints on municipal governments. Yet, cities actively intervene in enhancing the development potential of their land. Illinois cities adapted to constitutional restrictions in the late nineteenth century, just as cities today are adjusting to state and federal constraints. And just as cities in the last century decided either to take risks with their fiscal resources (debt and taxes) or not, and as a consequence either succeeded or failed in promoting their development capabilities, cities today choose to adapt to their changing fiscal and economic environments.

This very readable and reasonably brief history of local debt, which would provide an excellent historical case study in courses on urban policy and public budgeting and finance, offers a hopeful perspective that cities can choose to be instrumental in deciding their own fate. And Monkkonen concludes with the lesson that legal and constitutional constraints only modify city behavior and do not necessarily proscribe entrepreneurial and innovative development policies.

Creating Public Value: Strategic Management in Government. By Mark H. Moore. Cambridge, MA: Harvard University Press, 1996. 402p. $45.00.

Eugene Bardach, *University of California at Berkeley*

Although this book was not written to prove that public administration can be intellectually interesting, surely this is one of its most valuable accomplishments on a list of many such. Page one invites the reader to join Moore in thinking about some difficult subjects: the ethical responsibilities of public managers, the "diagnostic frameworks" they are to apply to settings in which they must execute their responsibilities, and the "particular kinds of interventions" that can "exploit the potential" latent in their varied settings. It turns out that the invitation is to join in a brisk climb from one well-argued paragraph to another with no let-up for 309 pages. Fortunately, because Moore is a systematic, lucid, and courteous writer, the pace is agreeable. And the view from the top is exhilarating.

Moore is not writing for academics but for practicing managers—though I address below some ways in which academic social science can profit from the analysis—and in particular for the sort of public-spirited and results-oriented managers who have participated in the executive training programs run by the Kennedy School of Government over the last twenty-plus years and in which Moore has been an active teacher. His arguments on the ethical responsibilities of managers are, in the main, not intended to give them responsibilities they have not owned up to but to legitimate their acting as creatively as they may have long wished to do. Their responsibility is to "create public value," says Moore, because their angle of vision, from the roles they occupy, permits them to see the latent potential for doing so. In particular, they can see changes in the demand for existing services (e.g., parents using the neighborhood public library as an ersatz day care center) and the transformational possibilities in existing resources were they to be used in new ways (e.g., police used for crime prevention, crisis response, and fear reduction as well as for crime control). This angle of vision will usually not be accessible to elected officials and to citizens, the more traditional sources of legitimacy for managerial action. Hence, if the managers do not seize the opportunities for value-creating action, the public will end up a loser, albeit an unwitting one.

Moore's signal contribution is probably to show how strategy and tactics can be enriched by ethical ideas, in a pragmatic and a philosophical sense. In playing the realist as well as the moralist, Moore stands Machiavelli on his head, so to speak. Managers should not merely accept accountability, they should embrace it, because doing so will usefully expose others in their organization to the same accountability demands. Managers should create new enterprises without prior explicit authorization and then subject themselves to "after-the-fact" accountability (p. 300). Indeed, if enterprises are complex and not easily described in advance of actually producing a working model, this may be the only realistic option. In embracing accountability, they should selectively scan the entire "authorizing environment" (pp. 118–20) for the mix of sources that can lend their actions legitimacy and hence authority. But because this can lead to demagogy, managers, though enterprising, must not be arrogant, lest they miss the opportunities to receive constructive advice and criticism from others as to how most effectively to carry on the enterprise.

Machiavelli is turned right side up again when Moore comes to appreciating the dynamics of action, and Moore is with him shoulder-to-shoulder. Machiavelli respected the swift current of "Fortune" that the prince had both to read and to ride. Moore writes of a manager "acting in a stream" (p. 290), which entails working with groups outside the manager's organization, balancing opponents and supporters within the organization, letting administrative systems trail the process of "persuading the organizations' employees to think and operate in a new way" (p. 291), capitalizing on and welcoming initiatives by others, and adjusting the pace of change so as to maintain momentum without fomenting rebellion. Unfortunately, Moore's appreciation of managerial dynamics is brief. Such an appreciation is so rarely essayed in the academic literature, though, that we must be grateful not only for its perceptiveness but also for its mere existence.

Can Moore's prescriptions of what managers *should* do be of interest to empirical social science? Machiavelli, too, wrote in a prescriptive mode; yet, in tribute to his broad observations and his seeming realism, Machiavelli is sometimes taken to be the first great postclassical empirical political theorist. Although no more self-consciously "systematic" in data collection than is Machiavelli, Moore offers prescriptions that also emerge from a wonderfully rich body of experience, the experience brought to him by thousands of executive training participants, hundreds of case studies in use at the Kennedy School as teaching materials, wide academic reading, and colleagueship with "distinguished practitioners" (p. 7) on the school faculty. He illustrates his prescriptions with the detailed discussion of six well-documented cases: William Ruckleshaus setting course at the EPA in 1972; Jerome Miller "de-institutionalizing" juvenile offenders in Massachusetts in the late 1960s; David Sencer of the CDC pressing for a nationwide swine flu vaccination program in 1976; Miles Mahoney as head of the Massachusetts Department of Community Affairs in the early 1970s trying to protect poor city dwellers from a politically popular Boston redevelopment project; Lee Brown, as chief of the Houston Police Department in the early 1980s, trying to reengineer the department along community policing lines; and Harry Spence reengineering the Boston Housing Authority when a state judge appointed him the BHA's "receiver" in 1980.

Moore's close analysis of these cases—comparing, contrasting, conjecturing about varied counterfactuals—partly resembles that of a geologist studying a rock specimen. Considered as a statistical datum, the rock is but one point in a large population. But considered as a medium in which underlying forces are known to be interacting with local conditions, it is a window on a developmental process. Some rock specimens, such as Martian meteorites in Antarctica, are much more remarkable as windows than their status as single data points might suggest. As sources of such developmental understanding, Moore's cases are not only well chosen but also well analyzed. I found the comparative analysis of the two "reengineering" cases the most illuminating and the ones that raise the intellectual quality of the book to its highest pitch. They improve the intellectual standard for what it means to "do case studies" in qualitative research generally, not just in public administration.

Removing College Price Barriers. By Michael Mumper. Albany: State University of New York Press, 1995. 304p. $24.95.

Anthony W. Morgan, *University of Utah*

Highly publicized increases in tuition charges over the past 15 years, a fundamental shift in federal policy from grants to loans, and the widening income gap between educational haves and have nots have produced a plethora of literature on college affordability. This literature ranges from popular magazine editions on college "best buys" to technical economic analyses of the causes of college cost and price increases. Michael Mumper's *Removing College Price Barriers* covers much of this literature but is distinguished in both its breadth and, to a lesser extent, its policy analysis focus. Mumper addresses four policy research questions. Why have college prices increased so rapidly? What actions have governments taken to keep college affordable? Why have these efforts failed to stem the tide of rising college prices? What policy alternatives are feasible?

Chapters of the book flow nicely from these four questions: (1) the benefits of a college education; (2) forces causing a rise in college prices; (3) changes in college affordability for different income groups; (4) the genesis and evolution of federal student aid; (5) the increasing costs of federal student loan programs; (6) a review of state efforts to equalize educational opportunity; (7) recent state innovations to assure access; (8) the effect of all these aid programs on college participation rates; (9) the failure to achieve college affordability; (10) an analysis of leading reform proposals; and (11) the author's proposal to achieve universal college affordability.

Mumper's argument starts and ends on an important policy premise embedded in Title IV of the Higher Education Act of 1965—equal access to a college of choice by students from families at all income levels. Like other Great Society programs that assumed sufficient wealth for guns and butter, the Higher Education Act of 1965 set ambitious policy goals of both universal access and choice of institutions. Mumper argues that achieving both of these original goals may not be viable in today's fiscal environment and that we should opt for the primacy of access of lower income students over choice among types of institution. This places him at odds with many of the more prominent writers from the private sector whose proposals Mumper criticizes as economically rational in their arguments of efficient resource allocation but politically irrational and thereby failing the test of both economic and political feasibility. Mumper's own proposal attempts to mitigate the failures of past federal and state "spread-rather-than-target-allocation strategies" (p. 227) by incrementally moving toward a greater degree of targeting student aid to lower income populations through a revitalized and entitled Pell Grant program coupled with reform of student loans.

In the genre of policy analysis (e.g., Weimer and Vining, *Policy Analysis*, 1992), Mumper gathers and synthesizes an enormous range of research and policy studies; analyzes the social, economic, and political context of the issues identified; and then evaluates specific policy alternatives. The work is an exemplar of policy studies usable by both policymakers and students of policy. It is accessible to the general reader, flows from a clearly organized structure, and is well written. The tone of the book also strikes a nice balance between "objective" analysis and advocacy of a policy direction. That balance is manifest, for example, in forthright acknowledgment of limitations in the analyses and in alternative explanations tendered where appropriate.

While breadth of coverage is a strength of the book, some important audiences are omitted, such as students in for-profit or proprietary institutions, graduate students, and part-time students, who now constitute more than 40% of those attending colleges and universities. Proprietary students are important because their higher loan default rates have affected policymaking, and they represent a level and type of training that is central to policy debates on education for a changing labor market—important components in the author's final proposal but not treated in the body of the work. The affordability of graduate education is increasingly important as students attempt to improve credentials to compete for the most lucrative jobs. The book's strengths of breadth and accessibility also mean some sacrifice in more detailed discussions of data and their definitions, which researchers might like to see.

A second limitation, characteristic of many policy analysis works, is a tendency to rely on a limited number of secondary sources. In general, Mumper does an admirable job of drawing from a wide range of the most important research studies. But in some areas, such as the causes of rising college costs, he relies too heavily on a few secondary literature reviews rather than draw more broadly from primary sources. Third, Mumper gives inadequate attention to assessing policies and incentives for institutionally based aid, which he acknowledges "mushroomed to $7.2 billion in 1992 . . . an increase of 160 percent over twelve years" (p. 35). Finally, there are some smaller editorial problems, the most important being the absence of a consolidated bibliography.

The literature on college affordability is a crowded field these days, which complicates the reader's choice. The Brookings Institution, for example, has produced a solid series of scholarly works that focus primarily on federal policies influencing college affordability (Hauptman, *The Tuition Dilemma*, 1990; McPeherson and Schapiro, *Keeping College Affordable*, 1991; and Gladieux and Hauptman, *The College Aid Quandary*, 1995). Halstead's solid analysis of tuition (*Higher Education Tuition*, 1989) as well as his continuing comparative state-level data (*State Profiles*, 1995) are data-rich resources for policy researchers. Occasional articles in the more scholarly higher education journals (*Research in Higher Education*, *Journal of Higher Education*, and *Review of Higher Education*) also offer important research and policy sources. Finally, the College Board and *Change Magazine* have published significant pieces that are intended more for general audiences.

One strength of Mumper's book is that it draws from all these, and other streams, in an attempt to give readers the broadest overview necessary for policy discussions at the federal and state level. For readers interested in an initiation into the range and dynamics of policy issues on this topic, Mumper effectively weaves in federal, state, institutional, and individual consumer vantage points. The other considerable strength of the book is its policy focus. Economists and student aid administrators have tended to dominate this literature, and their works emphasize economic and administrative policies. Political scientists as well as policymakers will appreciate Mumper's integration of these perspectives into a broader framework that examines underlying political forces at work and culminates in policy recommendations sensitive to this perspective.

Congress and the Rent-Seeking Society. By Glenn R. Parker. Ann Arbor: The University of Michigan Press, 1996. 172p. $39.50.

Patrick J. Sellers, *Indiana University*

U.S. election campaigns often attack corrupt Washington politicians, charging that they have been bought by special interests. The accuracy of these criticisms is not always clear. While scandals may reveal blatant corruption, it is often more difficult to tell whether most congressional incumbents are working to enrich themselves or are attempting to legislate solutions to the country's problems.

In *Congress and the Rent-Seeking Society*, Glenn R. Parker uses economic theories of rent-seeking to examine the extent of corruption in the U.S. Congress. The key to his argument is the motivation of legislators. Declining public respect for Congress has made it more difficult for members of Congress to enjoy the intrinsic, nonmaterial returns of their job: "the prestige and respect that accompanies elective office, the power and influence wielded by officeholders, and the visibility and prominence accorded political elites" (p. 3). At the same time, pervasive lobbying by interest groups has increasingly encouraged legislators to engage in rent-seeking activity. They use their legislative authority to intervene in markets to create advantages for particular interests, which in turn provide financial rewards (such as campaign contributions or honoraria) to the legislators.

Parker points out that these types of motivations are difficult to uncover through interviews. Politicians have every incentive to provide electorally advantageous answers to interviewers' questions, regardless of their accuracy. He therefore turns from the legislators' words to their actions, which are more difficult to conceal if electorally undesirable. He cleverly supports his rent-seeking arguments with this type of evidence, compiling extensive data on legislators' backgrounds and activities from more than 100 congresses. He finds that the decline in intrinsic benefits has discouraged careerist, service-oriented politicians from running for Congress. Instead, recent candidates are more likely to be noncareerist in nature, concerned more with financial gain.

As the composition of Congress has changed, campaign contributions from interest groups have increased. Members with more institutional power acquire more honoraria from interest groups. Such rent-seeking members tended to be more deeply involved in the House banking scandal, although this involvement affected their electoral fortunes only modestly. One would expect that rent-seeking behavior would be strongest after a legislator announces retirement, when reelection no longer constrains the pursuit of personal financial gain. Parker finds little difference in the level of rent-seeking behavior of retiring and nonretiring members. He argues that concern about postcongressional employment leads retiring legislators to avoid rent-seeking activities which might damage their reputation.

The rent-seeking theoretical framework allows Parker to conceptualize and find support for a number of counterintuitive but important observations about the evolution of Congress. He makes a convincing case for devoting more attention to the nature of legislators' motivations and changes in those motivations over time. But as is often the case with an innovative theoretical approach to a well-studied topic, several of Parker's arguments could have benefited from a more nuanced consideration of alternative explanations and evidence. He distinguishes between legislators motivated by intrinsic and material returns but devotes little consideration to the overlap of these motivations. The same qualities or skills may help legislators obtain both types of benefits. Serving as a committee chair or party leader can provide a legislator with prominence and personal satisfaction from legislative accomplishments. The same positions of institutional power, however, may be used to win honoraria, campaign contributions, or other types of rents. This overlap of motivations and positions is one aspect of Parker's analysis that merits closer attention.

When discussing campaign contributions, Parker notes that this aspect of congressional rent-seeking can produce numerous externalities, many of them negative. An additional externality that receives less attention is the role of campaign spending in providing information to voters. While campaign messages are sometimes inaccurate, ambiguous, or generally unhelpful, they also can provide information to voters for evaluating candidates. Recently elected legislators may collect more campaign contributions than their senior colleagues. But the high-spending legislators may also devote more resources to communicating with constituents and informing them of activities in Washington. In these ways, positive externalities may balance some of the negative effects of rent-seeking that Parker describes.

More generally, Parker portrays legislators as largely free from electoral constraints and mainly interested in obtaining rents. It is hard to separate this portrayal from one of electorally vulnerable legislators who solicit contributions out of fear for reelection. Parker argues that the high electoral margins of most legislators indicate a lack of vulnerability, and that legislators are not electorally vulnerable. But similar patterns may occur with a group of electorally vulnerable legislators who solicit contributions out of fear of defeat. With the decline of partisan identification, voting patterns and therefore electoral outcomes have grown more volatile. While 59% of the vote may have suggested electoral security twenty years ago, that same margin can indicate electoral weakness today.

Finally, it is important to consider how Parker's conclusions fare after considering the 1994 congressional elections and the 104th Congress. The House Republican leadership actively engaged potential PAC contributors, exchanging direct access to the legislative process for campaign contributions to party members. These legislator-dominated exchanges support Parker's arguments about the efforts of interests groups and legislators to obtain rents in their respective markets. At the same time, however, many of the newly elected noncareerist Republicans focused on improving the operation of Congress through campaign finance reform and government deregulation. Intrinsic benefits appear to be a strong motivator for these freshmen. The elimination of government regulations undoubtebly benefited their constituencies. But rent-seeking theoretically occurs when interest groups compete to obtain increased governmental involvement in their markets.

Parker's application of rent-seeking theory makes a significant and innovative contribution to our knowledge of the U.S. Congress. Applying new theoretical perspectives to well-studied topics often yields important insights, and Parker's work is no exception. Although portions of the analysis could benefit from a more nuanced consideration of alternative evidence and arguments, Parker draws our attention to the rise of rent-seeking in Congress, an important development in the institution's evolution. For these reasons, the book is strongly recommended.

The Psychological Assessment of Presidential Candidates. By Stanley Renshon. New York: New York University Press, 1996. 515p. $34.95.

Blema S. Steinberg, *McGill University*

In an innovative and pathbreaking book, Stanley Renshon provides a detailed analysis of how psychological assessments of presidential candidates can be done and why it is imperative that they should be done. As Renshon explains in his introduction, presidential elections increasingly revolve around issues of character and leadership reflecting growing public awareness that the character, judgment, and leadership qualities of its leaders count.

Renshon opens by analyzing the concept of psychological suitability and the type of ethical and theoretical dilemmas posed by such an assessment. In the first part of the book, he examines psychological suitability from the perspective of traditional concerns with the "mental health of presidents." In this context, two chapter-length case studies are presented: Barry Goldwater's 1964 presidential campaign and Thomas Eagleton's 1972 vice-presidential nomination.

During Goldwater's campaign, members of the American Psychiatric Association were asked to assess his mental health and psychological suitability. The low level of consensus and the wide diagnostic variability reflected, in Renshon's view, the biases and political preferences of the assessors. In the second case study, Eagleton's nomination and subsequent withdrawal on the grounds of repeated hospitalizations for depression, Renshon argues that two problems made his psychological suitability questionable. First, the length of his hospital stays could well have had a negative effect on his ability to carry out the responsibilities of office. That may have been true in 1972, but Renshon does not address the effect that antidepressant medications such as Prozac are having today in significantly improving the day-to-day functioning of depressive personalities, thus eliminating hospitalization. Renshon may feel that no one who is taking antidepressants could be a suitable candidate for president, but he ought to have addressed this issue.

Second, Eagleton's response to the situation also raised questions regarding his psychological suitability. He did not tell the McGovern campaign about this aspect of his medical history and, when the information leaked out, was clearly evasive in providing details. Renshon suggests that Eagleton's intense political ambition coupled with his low self-esteem produced this example of poor judgment, thus raising doubts about his future presidential performance.

To remove much of the subjective and political element in judgments concerning respective presidential candidates, Renshon offers a framework that links a theoretical approach to the study of character with an analysis of the important requirements for effective performance in the presidency. He argues that three aspects of character—ambition, integrity, and relatedness—are critical in influencing the caliber of presidential performance. While the choice of these three dimensions appears to be strongly influenced by Renshon's particular psychoanalytic perspective, that of self-psychology, and thus is open to charges of being somewhat restrictive in scope, these components do seem to be particularly suited to a character analysis of political leaders.

Renshon then formulates a theory of presidential performance that targets two basic tasks any incumbent of that office will face: leadership and decision making. In his discussion of the latter, Renshon emphasizes the importance of the quality of judgment exercised by an incumbent as opposed to the nature of the procedures followed and the technical "rationality" of the decisions taken. As for presidential leadership, Renshon notes that it should manifest itself in performing three tasks: mobilization, orchestration, and consolidation. Linking character and presidential performance, Renshon points out that each of these leadership tasks requires different skills and that each engages the three components of character differently.

To illustrate the above theoretical material, Renshon explores the role of character and judgment in two additional case studies: Gary Hart and Bill Clinton. Hart was forced to withdraw his presidential candidacy owing to revelations of an extramarital affair. Renshon is not interested in offering any moral judgments; rather, he asks "what is the connection, if any, between such relationships and presidential performance" (p. 246)? In a persuasive analysis, he suggests that Hart's conduct of this and other such relationships revealed flawed judgment that was the product of a narcissistic sense of entitlement. Renshon might also have considered an alternative hypothesis, namely, that Hart suffered from an unconscious sense of guilt which was manifested in self-sabotage and, in Freud's terminology, was one of "those wrecked by success" (vol. 14, 1914–1916, p. 316, 2d ed. 1957). In both cases, however, the prognosis for high-quality presidential performance would not have been strong.

In his analysis of Clinton, Renshon goes beyond his three dimensions of character to add the useful concept of character-based personality traits, such as "persistence," "empathy," "the need to be appreciated, even admired, as someone special," and "the wish to have it both ways" (pp. 288–96). While Renshon seems to have felt a need to enlarge upon his initial three dimensions, character-based personality traits are not part of his earlier theoretical chapter on character.

Renshon finds that Clinton's decision-making style, which gives each opposing party the sense that he agrees with them, is less a function of his need to be liked and more a product of his dislike of limits and his expectation that because he is special, he need not be bound by them. Renshon correctly observes that this is the hallmark of a narcissistic personality, which suggests his theory of character cannot entirely escape the more global constructs of the DSM IV categories of personality types.

This book is enormously valuable in providing other researchers with detailed checklists and appendices of questions that should be asked of any presidential candidate in order to measure ambition, integrity, and relatedness as well as leadership potential. Renshon's analytical framework, moreover, can be easily applied not only to presidential candidates but also to incumbent political leaders across a wide spectrum of political systems. Anyone interested in either the U.S. presidency or character analysis and its relationship to political performance would be extremely well advised to read this theoretically sophisticated and engaging study.

Second Coming: The New Christian Right in Virginia Politics. By Mark J. Rozell and Clyde Wilcox. Baltimore, MD: Johns Hopkins University Press, 1996. 285p. $32.95.

Duane Oldfield, *Knox College*

This book is a valuable contribution to the growing literature on the Christian Right. Major studies of the Christian Right have tended to focus either on public opinion and voting behavior or on national-level organizations. While the role of the Christian Right within state Republican parties is often commented upon, until recently it has not received adequate scholarly attention. *Second Coming* not only helps fill this gap in the literature but also has broader implications. Although Virginia is not perfectly representative of Christian Right/GOP interaction in other states (in particular, the Virginia GOP's reliance on conventions as a means of nominating candidates has allowed for greater Christian Right influence), many of the tensions between the Christian Right and other party factions in Virginia confront the GOP in other states and at the national level as well. (Those interested in Christian Right influence in other states would do well to look at *God at the Grassroots* [1995], edited by the same authors.)

Rozell and Wilcox argue that the Christian Right of the 1990s is qualitatively different from that of the early 1980s. They examine whether this "second coming" is likely to succeed where the earlier incarnation, associated with Jerry Falwell's Moral Majority, failed. Can the Christian Right work effectively with other factions of the GOP? Has it—and will it—show the pragmatism and willingness to compromise necessary to do so? The authors' answer is a qualified yes:

> By the early 1990s, a set of more pragmatic organizations had emerged that sought to build interdenominational alliances and to compromise when necessary to achieve their goals. The new organizations and leaders profess a commitment to pragmatism, although their actions have not always been consonant with that approach (p. 4).

As the authors take us through the recent history of Virginia politics, we encounter mixed evidence of Christian Right pragmatism. The book focuses particular attention upon the 1993 and 1994 campaigns. In these elections, the Christian Right played a key role in the nomination of candidates for statewide office and demonstrated its growing strength within the Virginia GOP. Its efforts did not necessarily pay off in November, however. In each election, the Christian Right helped secure the nomination of a controversial candidate who went down to defeat. In 1993, for example, Michael Farris, a home schooling advocate and former Washington state director of the Moral Majority, lost his race for lieutenant governor even as the rest of the statewide Republican ticket swept to victory. In the midst of the Republican landslide of 1994, Oliver North lost a Senate race to badly damaged incumbent Chuck Robb. In both races, Democratic opponents emphasized the candidates' ties to the Christian Right, prominent GOP moderates defected, and the Republicans lost a race that other nominees would likely have won. These races illustrate a pattern that the authors trace back to the "first coming" in Virginia: Christian Right pressure undermining the GOP by forcing it to adopt extreme positions and nominate extreme candidates.

The 1993 governor's race, however, turned out very differently. The Christian Right supported GOP candidate George Allen despite his incomplete opposition to abortion and did not pressure him into taking positions unpopular with the general public. Allen easily won election and, in office, has proved a strong ally of the Christian Right. In this case, the Christian Right demonstrated the pragmatism toward which the authors claim the "second coming" is moving.

In the end, the authors are unsure to what extent pragmatism and coalition-building will take hold. While the Allen campaign and recent moderation in Christian Right rhetoric point in the direction of pragmatism, the authors provide ample evidence to support an opposing conclusion. The Virginia GOP remains deeply divided between a Christian Right faction and a more moderate wing. Even the Allen campaign, the authors suggest, may have been a fluke. Christian Right "moderation" may have sprung largely from the fact that there was no more conservative candidate in the running. The reader may long for a more definitive conclusion, but the book's equivocation is consistent with the detailed evidence the authors present. The Christian Right exhibits contradictory features; any overall movement toward moderation and pragmatism exists in the midst of a variety of specific countervailing tendencies.

More problematic is the theoretical framework the authors use to organize their discussion. They attempt to distinguish between the Christian Right as a social movement, as an interest group, and as a party faction. Rozell and Wilcox see a long-term movement from a decentralized, poorly organized, and ideological social movement to more centralized, disciplined, and pragmatic interest groups and party factions. In their words, "much as interstellar gas and other matter eventually form a well-defined set of stars and planets, so the social movement of the Christian Right is gradually developing into a set of institutionalized interest groups and party factions" (p. 25). The imagery is suggestive, but the framework is not entirely successful. First, as used in this book, "social movement" too often becomes simply a synonym for disorganization and extremist behavior, and "interest group" a synonym for pragmatism. Second, as the authors admit (p. 16), a mobilized grassroots constituency is a key element of a social movement. Yet, by this measure, the "second coming" of the Christian Right looks more like a social movement than the first. Although the Christian Coalition has a far larger grassroots base than the Moral Majority ever did, the authors stress the interest group aspects of the coalition while characterizing the Moral Majority as a social movement. Third, any movement of the Christian Right in the direction of "institutionalization" appears to be exceedingly slow. Perhaps we should quit waiting for groups such as the Christian Coalition to transform from a social movement into an interest group or party faction. Instead of seeing them as "in transition" (p. 25) from one form to another, it may make sense to see both the "social movement" and "interest group" characteristics the authors identify as coexisting for an indefinite future within an integral whole which is the Christian Right.

The above problems notwithstanding, Rozell and Wilcox have provided a very valuable and sophisticated account of the Christian Right in Virginia. They skillfully lead the reader through the factional complexities of Virginia politics and show a keen sensitivity to religious and organizational differences within the Christian Right. Their analysis rests on an impressive base of interviews and survey data. The book is strongly recommended for those interested in the Christian Right, political parties, or Virginia politics.

Representation in Crisis: The Constitution, Interest Groups, and Political Parties. By David K. Ryden. Albany: State University of New York Press, 1996. 309p. $21.95.

Lane V. Sunderland, *Knox College*

David Ryden's book is about representation, the proper and constitutional role of political parties in U.S. democracy, and the influence of the Supreme Court in undermining this role. The book would enrich courses in U.S. government, political parties and interest groups, and constitutional law. Ryden analyzes group theory and pluralism, including the work of Arthur Bentley, David Truman, Robert Dahl, E. E. Schattschneider, Grant McConnell, and Theodore Lowi. He also devotes a chapter to group-centered representation, including the work of Iris Marion Young, Lani Guinier, and Robert Grady. He uses this analysis to inform his treatment of Supreme Court cases that have affected representation and political parties. Written in a readable and concise manner, the book successfully combines a basic and sophisticated analysis of representation with scholarship and theory on representation, political parties, and the Supreme Court. The result is a book which accomplishes the difficult task of laying basic groundwork in a thoughtful manner while advancing scholarship on the implications of important Supreme Court decisions and making an important contribution to an area that has been somewhat neglected.

One of the most appealing aspects of the book is Ryden's argument for political parties as the best means of reconciling individual and group interests to accomplish the constitutional aims of responsible and responsive representation. He does this in a thoughtful manner as he articulates the various functions representation and political parties fulfill. Yet, he avoids utopianism and recognizes shortcomings of parties as he presents his theory of parties as a part of the "art of the best possible" in our constitutional circumstances.

The primary problem the book emphasizes is that, in deciding cases affecting redistricting, campaign finance, ballot access, patronage, and party primaries, the Supreme Court has not recognized the important functions that parties fulfill. Ryden presents persuasive evidence to support his belief that the Court has undermined the healthy role political parties should play in our democracy. He argues that, by proceeding from a coherent theory of representation and the role for political parties and their accompanying structures (which he presents here), the Court could exert a healthy, formative influence on the operation of the political system (p. 2).

Instead of crafting its decisions in the areas of campaign finance, ballot and voter access, and party patronage to sustain or strengthen the role of parties, the Court has rendered decisions that seriously weaken the institutional foundations of political parties (pp. 182–3). Ryden identifies three aspects of the Court's theory of representation, each of which is deficient in recognizing the important role of political parties: (1) the individualistic perception of politics dominant in the one-person, one-vote reapportionment decisions; (2) the employment of group considerations in reapportionment decisions; and (3) the formal group representation in campaign finance decisions, in which the Court sought to control the financial contributions of groups in the name of equality of group influence (p. 127). When the Court is faced with a choice between considerations of uncurbed individual participation and an approach which sustains or strengthens parties, it inevitably chooses individual participation (p. 143). Ryden argues that political parties help resolve the contradictory demands of individual and group considerations in our system of pluralism (p. 182). Although somewhat crude and clumsy, parties operate in a way that is both representative and democratic (p. 124). They act as a means to reconcile the desired individualism with a recognition that the individual can be effectively represented only when acting as part of a group (p. 43). Unlike formal group representation along racial or religious lines, for example, which balkanizes society, parties assimilate different voices and interests and allow them to coexist. Ryden argues that political parties "are uniquely representative both of individuals and groups" because they funnel and mediate individual participation (p. 8).

Ryden's basic criticism of the Court's disregard for the role of parties in representative democracy is that the tenor of the Court's decisions emphasizes individual rights and subordinates the public good, as represented by group considerations which work their will through the mediating structures of political parties. His plea is for the Court to develop a theory of representation that includes the important values parties bring to democracy. Ryden's analysis is persuasive, notwithstanding the fact that the roots of egalitarianism and excessive individualism which lie at the foundation of many of the Court's decisions in this area go wider and deeper within society than the judicial decisions he critiques.

Ryden makes a strong case that the Court has discounted the salutary role political parties play in representative democracy. He believes that by adopting the theory of representation and parties which he sets forth, the Court can remedy this error as it decides important cases affecting political parties. It may be that the wiser course is for the Court, in most instances, to let policies stand if they result from political actions of the states or the nation, which themselves represent the actions of political structures and parties that Ryden believes should be supported.

Black Mayors and School Politics: The Failure of Reform in Detroit, Gary, and Newark. By Wilbur C. Rich. New York: Garland Publishing, 1996. 240p. $35.00 cloth, $18.95 paper.

Russell D. Murphy, *Wesleyan University*

At times it seems the nation's public schools are beset by the spirit of a perverse King Midas. There have been substantial increases in the real dollar amounts spent to educate the nation's young. But the more the nation spends, the less educated the young seem to be, and the less prepared for the real world they confront. Or at least this is a recurring theme in media assessments of the nation's schools and in the assessments of many others as well.

The state of the nation's school systems serves as the general backdrop for Wilbur Rich's case studies of black politicians and school policy. The book focuses on Detroit, Michigan, Gary, Indiana, and Newark, New Jersey, three cities where, Rich notes, a "new governing elite took over . . . the school systems" during the 1970s and 1980s (p. 3). Each city elected a black mayor, and in each "a coalition of black school board members, central administrative staffs, and teachers union leaders assumed control over the policy making apparatus" of the schools (p. 3).

Though notable, this changeover in leadership apparently made little difference educationally. "The new governing coalitions," Rich reports, "presented themselves as harbingers of fundamental change." What has followed, however, "has been an apocalyptic governing style that fails all involved, both the leaders and the public," and students as well (p. 3). Rich attributes the "failure of school reform under black leadership" (p. 4) to the workings of a "cartel-like governing entity" (p. 5) which controls public education in these cities. The cartel consists variously of school board members, professional educators, central administrative staffs, community activists, and teacher union leaders (pp. 5–6). According to Rich, this cartel is "incredibly skillful at staying in power" (p. 4) and uses its "control of school policy to promote the interests of its members" (p. 5), often at the expense of quality education.

There is much to recommend this study. The issues are as timely as they are troubling, and Rich's deep engagement with them is clear throughout. His work dramatizes the complex challenges still confronting the nation's public schools. Many of these are pedagogical—how best to improve the learning environment and classroom performance. But, as Rich stresses, many are profoundly political as well. There is simply too much at stake for it to be otherwise.

That "educational policy making is political" (p. 4) is a central theme in the study. Since so much is at stake, numerous policy minorities crowd the process, each trying to control budgets, contracts, jobs, and the curriculum. The competition is often intense, and as Wallace Sayre and Herbert Kaufman noted some years ago in their classic *Governing New York City* (1960), such competition increases the likelihood of stasis and stalemate. This, in turn, undermines faith not only in the public schools but also in the efficacy of education generally and, eventually, in the political process. Rich's study, then, is an important reminder that the stakes are not simply dollars or books, crucial as they are. At stake, more fundamentally, is the community's faith in its capacity to govern itself.

Comparative cases studies of this sort provide important perspectives on the workings of politics. This includes a lively sense of the roiled events behind, but never fully captured by, neatly formatted statistics published by government agencies or polling organizations. At the same time, neatly formatted statistics can add much to an analysis, and in this instance the study would have been strengthened considerably had there been summary data comparing the three cities with one another and with the nation. These data also could have been linked to the four stages into which Rich divides the evolution of black involvement in school politics. In any event, it would have been helpful to have systematic, longitudinal data on such matters as per-pupil revenues and expenditures, test scores, completion rates, and the demographics of boards of education, central administrators, instructional staff, union membership, and students.

It would have been useful, in addition, had the study dealt more intensively with fewer cases. The book is too far ranging. This is understandable. There is much to tell about these cities, and much that is of interest and significance far beyond their corporate boundaries. But in trying to cover too much in a relatively brief space, the discussions are somewhat hurried and sketchy. More needs to be known, for example, about "the protracted battles for control of the schools" (p. xi) and especially about the interplay between law and politics. Whether or not one finds the notion of a "public school cartel" useful, boards of education, superintendents of schools, and central administrative staffs are the duly constituted public school authorities. Even unions have a powerful legal claim on the system, which is, not so, typically, of mayors and other politicians. They are legally at some distance removed from this policy arena.

These are major constraints and long have been. Even so, there is a need for a more sustained analysis of the way black politicians, mayors among them, tried to reshape and reform the system and in what senses they are said to have "succumbed to the mighty forces of . . . the public school cartel" (p. xi). A more complete examination of these questions, moreover, would broaden our empirical knowledge of the policy process and the exercise of political power, making each a little less abstract and a little less mysterious.

More also needs to be known about individual reforms, beginning with a more discriminating definition of the term, beyond the general statement that "reform refers to efforts to achieve equity and excellence in the classroom through changes in school practice, personnel or structures" (p. 6). It would be instructive, as well, to know about the pedagogical and strategic thinking behind proposed changes and what backers expected to achieve. Were there reasonable grounds, for example, to expect Detroit's School Based Management initiative to succeed in a heavily unionized town, and did experience in other communities persuade Detroit's policymakers this program would markedly improve student performance? Similar questions may be asked about the Behavioral Research Laboratory experiment in Gary, with its goal of increasing reading and math scores, and the consequent dangers of "teaching to the tests." And it would be instructive, finally, to know more about the differential effect of elected and appointed school boards. Does this matter in terms of the stated goal of "equity and excellence in the classroom"? Was there any evidence one way or the other?

These are merely illustrative questions. The point is that the study would have been stronger still had fewer cases been pursued in greater depth. Case studies are well suited to micro-analysis. This is one of their advantages and one of their attractions. Rich's case materials are impressive, and he has a great deal to say about the many and important issues on which he touches. At the same time, this is an instance in which less would have been more, even if it meant saving some of the material for a second or even a third book.

From the Outside In: World War II and the American State. By Bartholomew H. Sparrow. Princeton, NJ: Princeton University Press, 1996. 354p. $45.00.

Edwin Amenta, *New York University*

It is a truism that war forges the parts of states central to prosecuting it. In *From the Outside In*, Bartholomew H. Sparrow claims that war also creates the domestic features of states, and he trains his analytical attention on a seemingly unlikely target: the American state during World War II. As he puts it, "the intranational American state . . . was systematically affected by the extranational factors of World War II and its aftermath. The American state was built from the outside in" (p. 4). In arguing that the war made U.S. domestic policy, Sparrow combats the conventional wisdom that such policy was more strongly affected by its period of enactment, in most instances during the New Deal. He pursues his thesis across the issue areas of social security, labor-management relations, taxation policy and public finance, and military procurement.

Sparrow's mission is explanatory as well as interpretive. His theoretical approach constitutes a kind of flank attack on politics by way of the sociology of organizations. Specifically, Sparrow extends the "resource-dependence" arguments of Jeffrey Pfeiffer and Gerald Salancik to the process of building the state. The latter he defines as encompassing the bureaucracy, the instruments of policy administration, and the established relations between the government and societal actors. According to resource-dependence thinking, organizations struggle to survive in uncertain environments and negotiate them in ways that protect their budgets and staff. Sparrow sees members of Congress and presidents and their administrations as motivated by a similar survival instinct. From this base, Sparrow advances several hypotheses about the effect of war on state-building. Their range encompasses the direction of state-building, the form of state-building, the extent to which wartime state-building persists ("ratchet effects"), and relative state-building. The hypotheses, sometimes complex, in turn provide expectations about differences in outcomes across the issue areas over time.

Here is how Sparrow applies the model to relative state-building, his most important subject. According to the postulates of resource-dependence, any exchange relationship works to the advantage of the partner with greater alternatives to acquire needed resources. State-building is thus held to be a function of the alternatives available to governmental and societal actors. The requirements of war alter the nature of alternatives in different ways across the policy areas. Sparrow argues, for instance, that during war the government has alternatives on the issue of income taxation and societal actors do not, and so income taxation is expected to grow during war. In social security, by contrast, both the government and recipients have alternatives, thus leading to lesser state growth. When the war is over, Sparrow expects the policy areas with the greatest number of constituents to expand the most.

Methodologically speaking, Sparrow is nothing if not resourceful. To appraise his hypotheses, he exploits comparisons across his issue areas and between the war and postwar periods. For the most part, his theoretical expectations are corroborated by his cases. What is more, *From the Outside In* constitutes another signpost of the advance of social science into the realm of historical technique. Distinguishing himself from many other social scientists, Sparrow does not dodge the grunt work that archival research can often be, and he uncovers material that will interest period specialists.

His interpretations are often subtle, so subtle that it is possible to miss the fact that Sparrow's thesis hedges strong and weak versions. The strong version—that the U.S. domestic state was built "from the outside in"—resembles Richard Titmuss's famous argument that war created the British welfare state. That version implies that World War II influenced domestic aspects of the U.S. state more so than did the period of their enactment or other periods. For the strong Sparrow thesis, moreover, the tough cases are labor relations and social policy. Claiming that war transformed taxation or military procurement is not nearly as controversial; each is more closely connected to making war. The weak version is that the war merely "affected" domestic aspects of the state, as suggested in the quotation at the top of this review. The book's title notwithstanding, the weaker version finds better support in the book's pages. For it is difficult to ascertain whether the war mattered more without a systematic comparison with the period that came before it. What is more, his resource-dependence argument implies the weaker thesis—major breakthroughs are expected only in income taxation.

Sparrow's case selection covers, as it should, the expected outcomes across most of his hypotheses. Yet, in the more difficult domestic areas, the cases are sometimes drafted in ways that appear biased. His discussion of social policy is conceptualized as "social security," which he interprets, as Americans do today, to mean old-age and survivors' insurance (OASI). But to most Americans around 1940 social security meant the other programs of the 1935 Social Security Act, especially means-tested old-age assistance and unemployment insurance. Before 1940 OASI had made no payments, but it is pressed into service by Sparrow as a stand-in for all social policy, which also included programs such as work projects. Social policy in the larger sense was retrenched severely during the war, whereas OASI survived it. Sometimes, moreover, Sparrow neglects to marshal cases of strategic importance for his theoretical claims, notably the wartime instance in which societal actors have alternatives, but the government does not.

For someone with my training it would be unseemly to complain much about a political scientist employing sociological theory. All the same, a few issues are worth raising. The idea of resource "alternatives" is not as readily applicable to politics as it is to organizations. More discussion about that would have been useful. Sparrow's resource-dependence hypotheses for peacetime are not nearly as well developed as those for wartime, but the wartime ones raise questions about generality. Certainly, the changes Sparrow documents happened during a specific war, but what did war generally have to do with these changes? Would all wars lead to the same outcomes? Although Sparrow addresses the issue with comparisons between World War I and II, it remains true that many aspects of the U.S. experience during World War II were specific to it. The war brought the United States extremely high employment and a resounding victory, as well as the rise to power of a Congress more conservative than any of the previous decade, which introduces a prominent casualty of the approach. That partisan politics is missing in action affords greater parsimony but at some cost. Often Sparrow theorizes generally about the relationship between the presidency and Congress, but his evidence draws from relatively liberal presidents and relatively conservative Congresses. Under different political circumstances the outcomes doubtless would have been different. For instance, after 1942, raising income taxes significantly was no longer an option for the administration. And that was due not to resource differentials but to changes in the partisan alignment in Congress.

I do not intend my sniping here to be read as detracting greatly from Bartholomew Sparrow's achievement. He has thought hard and boldly about theoretically important issues, has devised clever comparative and historical means to appraise his arguments, and has expanded our knowledge of state processes. In doing so, he has provided a new and provocative interpretation of an important episode in the history of the U.S. state. That his efforts cannot subdue all criticism should not conceal the remarkable gains he has made.

Shame and Humiliation: Presidential Decision Making on Vietnam. By Blema S. Steinberg. Pittsburgh: University of Pittsburgh Press, 1996. 397p. $40.00 cloth, $17.95 paper.

John P. Burke, *University of Vermont*

Shame and Humiliation is a well-written and well-organized analysis of three presidents—Eisenhower, Johnson, and Nixon—and the effects of their emotions, character, and personality on their Vietnam decision making. The argument is structured around, first, an examination of their respective childhood and adolescent experiences (with particular attention to relationships to parents and siblings), adult maturation, and career development. Steinberg then proceeds to their presidential career, focusing attention on three key decisions: Eisenhower's choice not to intervene at Dien Bien Phu in 1954, Johnson's escalation of the war in 1965, and Nixon's expansion of the war into Cambodia in 1969 and 1970. It is her contention, drawn from a psychoanalytic perspective, that each exhibited traits associated with a narcissistic personality; whereas Johnson and Nixon acted in a pathological manner, Eisenhower exhibited a "healthy narcissism" that enabled him to master his feelings and engage in a rational assessment of the decision at hand.

Narcissism is a basic component of human personality and a prerequisite for self-esteem. But for some, childhood experiences, particularly if parents single out a child as "special" and the object of lofty aspirations, can lead to narcissism that extends beyond the appropriate. Narcissists become preoccupied with self-importance and fantasies of endless success, but they are haunted by inner feelings of insignificance, of being "nobodies" unable to meet expectations and thus suffering the loss of parental attention or, later, the approval and respect of peers. Narcissists go through complex maneuvers to avoid being hurt but are highly vulnerable to shame and humiliation. When the latter occurs or fear of it arises, narcissists react aggressively; they seek to strike back, get even, and seek revenge.

For narcissistic political leaders, this can lead to emotion-laden, personality-driven use of force as a solution to policy dilemmas rather than a rational calculation of the costs and benefits of a variety of policy options. For Lyndon Johnson, the fear of being an unworthy successor to John Kennedy and the desire not to be perceived, particularly by the Kennedy holdovers in his inner circle of advisers, as a coward led him to escalate in 1965. Nixon, according to Steinberg, was an angry narcissist; his decisions to engage in secret bombing of Cambodia and, a year later, to invade that country "followed on the heels of foreign and domestic attacks that he deemed insulting and hurtful. Nixon chose Cambodia as the arena in which he could displace his anger" (p. 169).

Steinberg's biographical accounts of each of the presidents, plus the case studies of their respective decisions, are well crafted but cover familiar ground. Although she sometimes turns to archival materials (particularly the *Foreign Relations of the United States* volume on Indochina in 1954), much of her analysis relies on secondary sources. Vietnam junkies or those immersed in Eisenhower, Johnson, or Nixon scholarship will not find much factually new here. The strength of the work lies in the attempt to delve deeper into the personality of each president than others have attempted and to do so in comparative perspective. Having studied two of these presidents and their Vietnam decision making, I found her arguments persuasive at times and was particularly struck by the similarities—albeit with a different narcissistic twist—in her accounts of Nixon and Johnson. But I still remain cautious about ascribing motives to behavior that is often contradictory, subject to multiple interpretations, and just plain elusive at times.

In Johnson's case, his reluctance to engage in reprisal bombing (despite strong pressure) from summer 1964 up through the Pleiku incident in February 1965 seems less the product of his narcissism than of a president perplexed about what to do and a consummate politician aware of the political consequences of his choices. The crucial decision to send marines to Vietnam in March 1965 and to expand the size of their deployment and redefine their mission over the next three months was less the result of deliberative choice or a president fearful of humiliation than it was of incremental decision making within a very imperfect advisory system. Scholars who have delved into the rich archival material on Johnson's July decision (to send more ground troops but downplay the significance of that move) still credibly debate whether Johnson had made up his mind and just went through the motions of consultation or whether his extensive series of meetings with advisers that month had some effect on his decision making. In Nixon's case, the argument seems on stronger ground, although here the primary sources are less rich, and Steinberg is forced to rely on the reconstructions by Kissinger, Seymour Hersh, William Shawcross, and Tad Szulc.

That said, as both a political scientist and psychoanalyst, Steinberg brings her considerable talents to bear in offering an account that rises above some of the problems endemic in psychobiography. She does not overstate her claims and notes that her aim is not psychological reductionism or denigration of the critical importance of external reality in shaping decisions. Rather, her goal is to complement alternative explanations by understanding how wounded narcissists interact with environmental forces. Her account of how narcissists react to those around them is particularly interesting in expanding our understanding of the workings of presidential advisory systems. Narcissists are self-centered, and their emotional involvement with others is often shallow. They tend to surround themselves with sycophants and admirers, requiring from them a steady stream of adulation. Once no longer useful, associates are dropped with little concern. A sense of special entitlement, personal exploitativeness, disregard of the feelings of others, and an inability to sustain relationships also characterize the narcissist's dealings with others. In the conclusion to the book, Steinberg also analyzes Johnson's decision to seek peace in 1968 and Nixon's continued humiliation by Thieu and the North Vietnamese in 1972, which led to the Christmas bombing and the eventual Paris agreement. Steinberg holds that Nixon, having failed to achieve his much vaunted "peace with honor," satisfied his narcissism and bolstered his self-esteem by inventing one.

Overall, Steinberg's account is thoughtful and provocative. It represents a welcome addition to the continuing debate on Vietnam decision making.

Religion in Public Life: A Dilemma for Democracy. By Ronald F. Thiemann. Washington, DC: Georgetown University Press for the Twentieth Century Fund, 1996. 186p. $17.95.

Luis E. Lugo, *Calvin College and Center for Public Justice*

One of the more notable achievements of the Christian Coalition and other newly emergent religious conservative movements has been the reintroduction of religion and religiously based convictions into U.S. politics. These groups have been so successful in this regard, in fact, that for most citizens—if not for most within the academy—the question is no longer whether religion belongs in the public square but what form this involvement should take. Much of the discussion on this topic has taken place among conservatives, but some within the liberal community also have begun to rediscover the religious roots of their own tradition and to question the strict separationist dogma which has come to prevail in their circles.

This highly readable book by the dean of Harvard Divinity School falls into this category of "revisionist liberalism" and can best be read as a plea to modern political liberals to take religion seriously as an important and positive factor in U.S. public life. Its argument in sum is that the attempt to exclude religion from the conversation of democracy is intellectually insupportable and practically impossible. Thiemann is not unaware, of course, of the potential dangers. He readily admits, for example, that religion has often been a divisive force in politics and that there indeed are compelling historical reasons behind liberalism's attempt to contain religion within a private sphere. Yet, he insists that liberals must recognize that religion has played a crucial role in shaping our political culture, and the attempt to remove it from the public square would not only violate the religious character of the American people but also, given the close connection between religion and morality, undermine our attempts to address the country's crisis of public morality. Due largely to its double-edged character, the question of religion's appropriate role in public life remains, as the subtitle indicates, a dilemma for U.S. democracy. One does not have to share Thiemann's Lutheran background and its highly developed sense of cultural paradox to appreciate the ambiguous nature of religion's public role.

If we are to engage in a serious and much needed debate on the proper role of religion and to harvest its contribution for the renewal of U.S. public life, then Thiemann suggests that we start by clearing away the rubble of First Amendment jurisprudence which has piled up since the Supreme Court's *Everson* decision fifty years ago. In a couple of tightly argued chapters he helpfully examines the Court's often incoherent and arbitrary rulings, its undue reliance on the metaphor of the wall of separation between church and state, and its uncritical use of such concepts as accommodation and religious neutrality. He sensibly urges us to move beyond these suspect terms and develop a coherent framework that shows how both religion clauses of the First Amendment serve the same end of religious freedom. These clauses, he correctly observes, were not designed to prohibit the expression of religious views in our pluralistic democracy but to prohibit the state from favoring any particular form of religious belief or practice.

The challenge for the liberal tradition, as Thiemann explains in the core chapters of this volume, is to get beyond the wall of separation and the myth of neutrality and espouse a "virtuous liberalism," one that appreciates the need to nurture a robust sense of civic obligation and citizenship. He casts his position as a revisionist challenge to modern liberalism, with its emphasis on government neutrality and secularity and its exclusive focus on individual freedom. Thiemann cautions, however, that this should not be confused with the sectarian communitarianism of people like Alasdair MacIntyre and Stanley Hauerwas, which in his view is more effective as a critique of modern liberal theory than as a genuine alternative to liberal political practice. He looks instead to such thinkers as Jean Bethke Elshtain, William Galston, Michael Walzer, and Charles Taylor, who while affirming the centrality of liberal values (individual liberty, equality, and toleration) also recognize the need of democratic societies for a virtuous citizenry imbued with a sense of the common destiny and committed to the common good. In Thiemann's view, religion not only is compatible with this kind of liberalism but also is its indispensable ally.

Thiemann's argument is very powerful but, alas, not quite complete. Unless Thiemann is willing to develop it further, he is not likely to assuage the concerns of those who fear that the reintroduction of religious arguments into political discourse will only revive a kind of factionalism that is destructive of democratic politics. This fear, one should add, is not the exclusive preserve of secular liberals; it exists as well among many religious people and across the political spectrum.

The problem is that while Thiemann has helpfully clarified the distinction between private and public, and correctly shown why religion cannot be confined to the former, he does not adequately define the meaning of the political. Thiemann correctly argues that the realm of the political is not coextensive with the public—a space which is also occupied by the diverse institutions and associations of civil society—but he fails to specify the proper distinction between them. The challenge for any public philosophy, however, lies precisely in drawing this distinction and showing how the state should respond to the built-in pluralism that characterizes civil society. Absent such a framework, one is left without a clear sense of the proper nature and scope of politics and thus of the kinds of questions we can legitimately seek to resolve within the political realm.

In an attempt to make up for the lack of a principled institutional limitation on the political claims of religion, Thiemann turns to a discussion of the degree of certainty which religious believers should ascribe to their core theological affirmations. This question is important in its own right but is largely beside the point. To accomplish the end Thiemann seeks we do not have to convince devout believers of the nonabsoluteness of their faith claims (something which may not be possible in any case), only that the resolution of many ultimate questions falls outside government's limited role. I think Thiemann would agree that our pluralistic democracy would be well served if believers of all persuasions had absolutely no doubt about that.

Comparative Politics

Israel in Comparative Perspective: Challenging the Conventional Wisdom. Edited by Michael N. Barnett. Albany: State University of New York Press, 1996. 296p. $59.50 cloth, $19.95 paper.

Abdalla M. Battah, *Mankato State University*

This book grew out of the conference "Is Israel Unique?" held at Hebrew University of Jerusalem in June 1993. Two presumably related issues form the basis of this book: the uniqueness of Israel and Israel's standing in comparative social sciences. The book's underlying assumption is that comparativists who hold the view that Israel is a unique case avoid studying it, thereby causing the neglect and marginalization of Israel in comparative studies. Hence, the mission of the volume's ten collaborators is (1) to "testify to Israel's normalcy" (p. 7) and (2) using comparative concepts and frameworks, to demonstrate the usefulness of its study for theory building and/or application.

The book is divided into an introduction (chapter 1), two main parts, and a conclusion (chapter 10). Part One, "Israel and International Politics," contains four chapters, by Shibley Telhami (on foreign policy), Gabriel Shaffer (Jewish diaspora), Mark Tessler and Ina Warriner (gender and war/peace issues), and Michael N. Barnett (economic development).

Part Two, "Israel and Domestic Politics," also contains four chapters, by Ian Lustick (on Jerusalem), Joel S. Migdal (society formation), Rebecca Kook (national identity), and Gershon Shafir (Zionism and colonialism). The conclusion, by Yehezkel Dror, reflects on the notion of uniqueness and its multiple readings.

In the introduction, the editor, Michael Barnett, provides an overview of the "marginalization" and "uniqueness" problems. He identifies three primary "culprits" for the marginalization of the Israeli case: comparative social sciences, Middle Eastern studies, and scholars of Israel. Comparative studies underwent an epistemological and methodological shift in the 1970s (from modernization to dependency and world-system perspectives) that undermined the Israeli case. Middle Eastern studies ignore Israel because the region is treated as an essentially Arab-Islamic one and because of political and crisis-driven concerns. Many scholars of Israel are no less guilty by their reluctance, due to ideological or religious beliefs, to situate Israel in comparative studies. Since Israel's uniqueness is viewed as a virtue by such scholars, theory building and comparability with other countries are of little consequence.

The main chapters discuss interesting topics and contain a wealth of useful information. Little attention is paid in each chapter to the twin problems of uniqueness and marginalization as such, since the task is left to the editors' introduction. Each chapter independently serves as an illustrative example on the study of Israel. The quality of analysis is uneven across the chapters, and some, to be sure, suffer from serious flaws. The book's only empirical chapter, by Tessler and Warriner, using public opinion data from Israel and Egypt, attempts a comparative analysis of the relationship between gender and issues of war and peace. But the data are limited, old (1989 for Israel, 1988 for Egypt), and unrepresentative. The age of the data might not be so important elsewhere but is germane here, especially since we would expect attitudes (of both sexes) to reflect the effect of the Arab-Israeli peace agreements of the last three years. Also, Egyptian data "represent" only adult Muslims; in the case of Israel, it is not clear whether the settlers on the West Bank were included. The exclusion of the settlers, most of whom are hard-liner expansionists, could skew the findings—possibly significantly.

Kook's chapter is the least convincing. It focuses on national identity in Israel in order to provide a rationalization for Israel's exclusion of the Arab minority. She argues that notwithstanding their possession of Israeli citizenship, Israeli Arabs (comprising 20% of the population) can be legitimately excluded on the basis of "fundamental requirements of democracies" (p. 201). This practice, she maintains, is also common to other democracies, including the United States, the United Kingdom, and France. The trouble with this line of argument prima facie is that it privileges misguided "democratic" practices over principles and ideals of democracy. It further ignores, for example, the struggles of the U.S. Civil War, which led to three important constitutional amendments relating to the legal and political status of African Americans. True, discriminatory and exclusionary practices continued (e.g., Jim Crow laws), but African Americans had remedies in government action and, ultimately, the Constitution. That is not the case in Israel, where exclusionary practice is legal government policy, systemic and systematic, as Kook points out with respect to three key issues: "Law of Return," property rights, and civil rights. Like the native South African living under the apartheid system, the native Arab in Israel has no genuine recourse to the law or the government on matters of national or political rights.

Some of the historical information in the book is inaccurate and misleading. An example is Barnett's claim that mandated Palestine was "thinly populated" and had harsh soil that "limited the country's potential exploitation for agricultural production" (p. 119). But he provides no empirical support for this. Indeed, scholarly evidence proves the opposite with respect to both population density and land fertility. On the latter, consult the authoritative study of Walter C. Lowedermilk (*Palestine's Economic Future: A Review of Progress and Prospects*, 1946), which favorably compares Palestine's agricultural potential with that of southern California, no less.

Some of the particulars cited above as shortcomings are admittedly peripheral to the larger project. The core concerns, as stated earlier, are marginalization and uniqueness, and on those the book fails dismally. First, the marginalization problem is overstated and improperly handled. Assessment of Israel's marginalization is, after all, an empirical matter, but the volume does not treat it as such. If it did, presumably the verdict would have been based on relevant data collected and analyzed—perhaps in a comparative perspective, along with, say, Jordan or Syria. My hunch is that far from being marginalized, the Israeli case overall receives relatively more (positive) attention than do many others in the region.

What about the "normality" of Israel? Again, the case was not made effectively, and the concluding chapter testifies to this failure. Strangely, Dror argues just the opposite of all others in the volume: "Not only is Israel unique as a matter of fact and from a comparative perspective, but its main belief systems consist of a dominant ideology which regards itself as unique" (pp. 246–7). Furthermore, Israel's uniqueness must be seen as an existential as well as ideological asset. To him, the challenge to Israel's uniqueness by comparative social scientists, post-Zionists, and others represents a grave danger to the country, one which must be met with "vigorous steps to re-Zionize Israel" (p. 261).

The inclusion of this chapter is, therefore, rather curious, making one wonder whether it indicates ambivalence in the editor's mind. Indeed, such ambivalence may be detected by the careful reader early on (p. 8), where the editor writes: "Even if Israel does exhibit some atypical qualities—and it does—this does not justify its exclusion as a case." Well, amen! There is much merit in treating Israel (indeed, all other countries) in comparative perspectives. But it is difficult to understand such a statement in view of the fact that a conference as well as a scholarly volume were devoted to demonstrating the "normality" of Israel. Is this not like wanting to have your cake and eat it, too?

Although this volume does not fulfill its mission, it contains scattered insights and modestly contributes to on-going intra-Jewish debates. Pragmatically oriented readers may find Lustick's chapter on the thorny issue of Jerusalem politically refreshing and provocative.

Business and Democracy in Latin America. Edited by Ernest Bartell and Leigh A. Payne. Pittsburgh, PA: University of Pittsburgh Press, 1995. 292p. $49.95 cloth, $19.95 paper.

Paul G. Buchanan, *University of South Florida*

Scholarly interest in regime transitions and consolidation has shifted from studies of the dynamics of regime change to the role played by sectoral agents, interest groups, and political actors under various emergent regimes. In this volume, the editors and contributors explore the role of business under the electorally based regimes that now govern six important

Latin American countries: Argentina, Bolivia, Brazil, Chile, Mexico, and Peru. Given the nature of politics in several of these countries, the "democracy" in the title is somewhat overstated, but the need to look at the role of business is not. For in the current neoliberal climate governed by the so-called Washington Consensus, capitalist consent is most assiduously pursued by elected governments, regardless of the detrimental effects this may have for the prospects of "deepening" democracy beyond its electoral and parliamentary foundations.

Bartell and Payne argue that business elites in these countries have become committed democratic actors who no longer seek the protection of authoritarian regimes and state intervention when their political and economic interests are challenged. The editors maintain that business played a critical role in undermining the authoritarian regimes in the 1970s and 1980s and that now, even as business elites pose a latent threat to democracy because of (paradoxically) their self-perceived strength or weakness, they will support democratic governments that consistently enforce "fair" rules of capitalist competition, limit state intervention in the economy, and uphold property rights. This supposedly allows adroit government leaders to pursue divide-and-conquer strategies vis-à-vis both business and other social groups within the framework of neoliberal economic competition, thereby promoting economic efficiency while preventing any one group from holding a determinant position in the affairs of state.

The editors write that it seems somewhat surprising to find business, given its prior support for dictatorships, exercising a decisive leadership role in the current political milieu. With few exceptions, they avoid the issue that most of the emergent entrepreneurial sectors did not merely "survive" (the editors' term) the dictatorial episodes of the 1960s, 1970s, and 1980s but in fact prospered and were structurally buttressed by authoritarian economic projects that, mutatis mutandis, have been continued by their elected successors (Mexico being the exception). As chargers of interest, conduits for foreign investment, and arbiters of the national economic climates, these increasingly transnationalized oligopolies now occupy as never before a dominant position in their respective political-economic systems, thereby accentuating the structural dependence of these states on capital in ways alluded to (but perhaps never imagined) by Lindblom, Offe, Przeworski and Wallerstein, among others.

Nevertheless, the editors argue that business elites remain incapable of exercising hegemonic leadership, even given the ideological supremacy of free-market principles the world over and their position as the decisive nucleus of economic activity. Instead, "while capable of defensive collective action, business leaders . . . lack the cohesion to formulate and implement policy and they are susceptible to artful political strategy and negotiation by government officials" (p. 284). This is a variant on what Guillermo O'Donnell once called the "impossible game," or what Juan Carlos Portantiero called a "hegemonic impasse or stalemate" (empate hegemonico): a situation in which actors can veto the preferred strategies of others but cannot impose their own. The twist is that in this case the conflicts are between entreprenurial elites and governments, not between socioeconomic classes or political parties, and the editors feel that governmental elites can gain some measure of advantage in such a situation. History suggests otherwise.

Perhaps individual capitalist fractions cannot exercise hegemonic leadership and impose their will upon all others, and perhaps there is not a coherent project that acts as a factor of cohesion for the capitalist classes as a whole (both disputable notions), but Latin American business nevertheless has a virtually unprecedented say in the economic and political life of the region. Not only have domestic entrepreneurs found elected regimes to be compliant partners in the transformation of their respective societies, but also they are aided and abetted in these projects by an international business community that has consistently pressured for the opening of domestic markets and a down-sizing of the state as a condition for foreign investment, as well as by the structural and social weakness of traditional subordinate group collective agents, such as the trade union movement. Hence, reinforced by the authoritarian economic projects, bolstered by the support of the international business community, allied with the political elite in a marriage of mutual convenience under the guise of capitalism's "best possible political shell," ideologically unchallenged by subordinate groups and their collective agents, and no longer needy of military protection, perhaps it is more appropriate to say that business constitutes the new "power bloc" in Latin America.

Conaghan argues that in Bolivia (p. 130) none of the trade-offs between workers and capitalists that are considered the basis of a class compromise in democratic capitalist societies currently exist, and given the low public perception of the business elite, capitalist hegemony has not obtained in that case. But working-class consent may not be needed, or at least not needed over the short term, for the interests of business to be realized under procedurally democratic conditions. However obliquely, virtually all the case studies point out the same features: Weakened by the compound assaults of authoritarian repression, economic crisis, and the changing structure of production in these states, the laboring classes increasingly are atomized as citizens and subject to the immediate imperatives of survivalist alienation. With their sectoral agents curtailed, and with their political vehicles relegated to a secondary status in the parliamentary arena (the exception being the Workers Party [PT] in Brazil and various elements of the Mexican trade union movement), members of subordinate groups are reduced to pursuing their individualistic material interests within the neoliberal parameters as presently given. Under such conditions, business can afford to be democratic, if not hegemonic.

Carlos Acuna notes that in Argentina it was the combination of military extremism and ineptitude, followed by the subsequent shift toward the entreprenurial sector on the part of the Radical and Peronist governments of Raul Alfonsin and Carlos Menem, respectively, that made democracy the most viable political form for the bourgeoisie. In particular, Menem's repudiation of all the traditional planks of the Peronist platform, his virtual "colonization" of key cabinet posts with well-known business figures (a tactic repeated in virtually all the other cases studied here), and his successful divide-and-conquer strategy against organized labor (something attempted unsuccessfully by the Alfonsin administration) gave business the confidence to assert its support for political democracy over the long term.

Bartell confirms this scenario in Chile, where business power has been magnified by the extraordinary success of its new entreprenurial elites in exploiting the market openings provided them under the umbrella of Pinochet's repression. Using comparative interview data to contrast entreprenurial attitudes and perspectives in Chile and Brazil, Bartell finds that the successful application of the neoliberal model in the Chilean case allowed the business elites who survived the economic crises of 1976 and 1982 to be confident that a return to democracy did not mean a return to the statist economic projects of the past. Here, consistency of national economic policy was predicated upon proven success in international market competition, something not evident in Brazil. Hence Chilean entrepreneurs were more confident that their interests would continue to be well served regardless of the ideological inclination of particular governments, something reaffirmed by the Aylwin (and now Frei) government's support for the neoliberal model. This stood in contrast with the perceptions of Brazilian entrepreneurs, who were doubly vexed by inconsistent government policy edicts and the dangers of opening the capital-intensive and corporativistically organized domestic market to full international competition.

Payne amplifies the argument about Brazil by pointing out that the combination of previous military mismanagement, international ethical pressures, and the relative degree of latitude and room for maneuver afforded business by elected government eventually outweighed capitalist insecurities and the propensity to seek authoritarian shelter in uncertain times. The relative degree of business unity (notwithstanding regional differences and the internecine quarrels within the major industrial confederation, FIESP), coupled with the structural weakness of organized labor and other subordinate groups, allows Brazilian capital to be flexible enough to support the electoral regime while simultaneously preventing it from engaging in significant socioeconomic redistribution programs (such as land reform and social welfare projects). As a result, Payne claims that Brazilian business occupies a "privileged position" in government that allows it a disproportionate influence over political outcomes (p. 240).

Durand demonstrates the importance of capitalist unity—particularly in its use of collective agents—in his study of Peru. According to him, it was the formation of a single peak association representative of capitalist interests, CONFIEP, that "changed the ability of business to influence policy and political decisions . . . in unison" (p. 173). Even so, he argues that an inherent lack of class unity continues to simmer below the surface of trade association agreements, and this could undermine business confidence in being able to realize its interests within the electoral regime. Nevertheless, the emergence of a dominant collective agent forged a previously unseen unity of perspective within the Peruvian economic elite, and provided with a supportive political climate by the Fujimori administration, business has been able to pursue vigorously its interests without challenging the electoral bases of the civilian regime.

The ambivalent attitude of business toward democracy is best summarized by Heredia in her analysis of its relationship with the Mexican state. Focusing on the interaction between Mexican political and economic elites, she concludes that "business leaders have thus far succeeded only in ushering in partial and segmented political liberalization that has tended to reinforce, rather than erode, the basic pillars of authoritarian rule" (p. 180). The incorporation of the major business-backed opposition party, PAN, into the electoral system and the rapprochement of key capitalist groups with the political elite after a period of stress in the early 1970s and early to late 1980s were processes that "contributed to an overall political liberalization within existing political institutions, rather than outside. This has ensured political stability but has simultaneously reproduced some of the key mechanisms that have historically shored up authoritarian rule" (p. 208). So long as their interests were accommodated within the existing parameters, Mexican elites were content to play within flexible authoritarian rules rather than push for genuine democratic reform. So the lesson to be learned, one that was lost on the military dictators in other countries, was that flexibility in catering to business concerns is critical for its continued support. The PRI regime and its elected counterparts in the other five cases are acutely cognizant of this.

Only in Conaghan's treatment of Bolivia is explicit reference made to the rewriting of the "public transcript" of capitalism in the Western Hemisphere over the last decade or so. As both a case for the ideological supremacy of capitalism as a social form and as an instrument for unifying business interests in a common project, the current public transcript (as defined by James Scott), both in the United States and Latin America, is to reassert the political, economic, and ideological strength of the entreprenurial classes in the postauthoritarian moment. Even if the project remains incomplete in Bolivia, and even if it has served to expand rather than erode the bases of the PRI regime as well as bolster the dirigiste tendencies of a number of Latin American presidents (Fujimori and Menem foremost among the cases presented here), it is clear that these are the best of times for capitalists in the region.

What emerges from this compilation is sobering (the editors' generally optimistic assessment notwithstanding). In Latin America today, business is the political equivalent of a 500-pound gorilla: inherently antagonistic toward sharing its privileged position with subordinate members of the community, even if docile and self-assured of its place in the social hierarchy when operating from a position of acknowledged strength, but ultimately very dangerous when it perceives itself to be weakening, chooses to demonstrate its power, or is seriously threatened. Thus, under elected regimes in the Western hemisphere they may now wear suits rather than uniforms, but in the end, gorillas still rule. For students and scholars with an interest in the prospects for deepening democracy in the region, this book is a compelling reminder of that fact.

The Logic of Evil: The Social Origins of the Nazi Party, 1925–1933. By William Brustein. New Haven: Yale University Press, 1996. 235p. $27.50.

Jeffrey Kopstein, *University of Colorado at Boulder*

Why did people become Nazis? Based on a mammoth sample of pre–1933 National Socialist Party members drawn from the files at the Berlin Document Center, William Brustein makes the connection between socioeconomic position and the decision to join the party. In doing so, he argues that the choice to join was based primarily on individual calculation of material interest. It was rational. The party program contained specific proposals to deal with pressing economic concerns of key social groups, and the organization effectively used selective incentives to overcome formidable barriers to joining.

Needless to say, this thesis is controversial. Calling Nazis rational materialists runs against the bulk of recent historical scholarship, which has increasingly stressed the centrality of racial and biological thinking in accounting for the Nazi phenomenon. Brustein, in fact, explicitly rejects anti-Semitism and irrationalism as helpful in understanding the social origins of the movement. It is certainly one of the strange things about history and social science that Michael Burleigh and Wolfgang Wippermann (*The Racial State,* 1991), Daniel Jonah Goldhagen (*Hitler's Willing Executioners*, 1996), and Brustein are all talking about the same country. Yet, they are.

Brustein tells a fascinating story of how ordinary Germans joined an extraordinary party for ordinary reasons. The structure of the book is quite sensible. First, he summarizes and criticizes the extant approaches to the social origins of Nazism, including the irrationalist/mass society approach of

Arendt, Kornhauser, and others; the Lower-Middle-Class-Reaction thesis associated primarily with Lipset; the confessional-social milieu argument according to which Catholics and Social Democrats were somehow immunized from Nazi affiliation because of their social attachments; and, finally, the notion of the Nazis as a general catch-all party of protest after 1928.

Brustein's criticisms of these explanations are both conceptual and methodological. On the irrationalist/mass society thesis, he points out that Nazi membership fluctuated greatly between 1925 and 1933. Many people who joined the party before 1933 (40%) also left before 1933. "If the irrationality thesis is correct, then we must assume that thousands of previously irrational people suddenly became rational by 1933" (p. 10). This is an interesting point, but it is also true that the mass society thesis would have no problem explaining wild fluctuations in the membership of political organizations; that, after all, is part of what the notion of social anomie is all about. On the idea of Nazism as a revolt of the middle classes, Brustein correctly points to recent work on voting patterns in the late Weimar era which convincingly shows that the Nazis had made surprisingly large inroads among the working class and that their middle-class support was by no means universal. To the social milieu argument he retorts that if German Protestants were available for recommitment away from democratic politics, why did they choose the Nazis over other extremist organizations? Similarly, on the idea of the Nazis as a catchall protest party, he questions why people supported the Nazis rather than drift into other parties. His main methodological criticism of the voting studies is familiar to students of the late Weimar period: Despite valiant efforts to overcome the ecological fallacy, the absence of individual-level voting data makes it difficult to say with any assurance who exactly liked the Nazis.

Brustein wants to address both these problems, the conceptual and the methodological, by turning the focus of analysis to the people who actually joined rather than simply voted for the party. Instead of conceiving on National Socialism as a political residual into which the discontented and unattached could drift, Brustein wants to go one step farther and give positive reasons for Nazi support. In several chapters compared the Nazis with other mainstream parties, he maintains that the Nazi program was widely known and contained specific appeals to the groups of people who eventually joined in unusually large numbers. On the basis of his huge sample, Brustein reconstructs individual-level data for party joiners. The central argument is that those socioeconomic groups (both farmers and workers) that were most threatened by import competition and unemployment looked favorably on the protectionist and Keynesian rhetoric of the Nazis. The political immunization against Nazism that most historians have found among Catholics and attribute to the strong social milieu of the Catholic Center Party and political Catholicism in general is attributed by Brustein more to regional difference in modes of farming and systems of land tenure. Instead of explaining why Protestant farmers drifted to Nazism by reference to the weaker immunizing effect of their cultural milieu, Brustein finds that these differences in culture actually mask a deeper split in modes of production that the Nazis could exploit in their programmatic statements.

Brustein is able to make these fine distinctions by dividing up the German population into highly specific occupational groups and import/export sectors. One may quibble with how he parses the population into jobs and sectors and even suspect that alternative classifications might not have so strongly supported his data. Still, this analysis of who joined the National Socialist Party is unsurpassed in its sophistication and methods. Anyone who wishes to challenge his interpretation of who joined the party will have a lot of work to do.

I suspect, however, that this work may be worth undertaking. If Brustein were telling a story of who voted for the Nazis I might be more convinced of the rational materialist logic. Pocket-book voting is a widely (if not universally) shared tradition in electoral research. But *joining* was not the same as voting for the NSDAP, and even after 1925 it was not an easy decision. Although he wants to treat the Nazis as an ordinary party like any other, even in 1925 everyone knew that this party was essentially anti-systemic. The decision to join the Nazis was simply not the same as becoming a member of the Catholic Center or the German People's Party.

Brustein knows this and devotes an entire chapter to selective incentives (SI) that the Nazis employed to overcome the disincentives to joining. From an empirical standpoint, the discussion of SI is important in order to account for decisions to join or not join that are not predicted by the theory. I found this chapter to be the weakest in an otherwise well-crafted book. Although the idea of SI follows from the intellectual approach taken here, speaker's fees and other small goodies doled out by the Nazis (and every other party, for that matter) simply cannot carry the empirical or theoretical weight that the author wants them to. It is true that after the Nazis came to power they had plenty of booty to spread around, but before 1933 they did not. The Nazi solution to the collective action problem did, in fact, rely on rationality, but it was a rationality conceived of more broadly than this book does. "Territorial expansion and racial persecution" (which Brustein claims were a "hidden agenda" [p. 182] and therefore not important before 1933), especially for the kinds of people Brustein describes—party joiners—were rational alternatives to normal occupation activity in the economy.

These thoughts aside, Brustein has written an important book. The database is impressive, the theory is provocative, and the conclusions are scary. If he is right, and other racist and xenophobic parties can mask their true aims until they attain power, then evil may simply need to craft its message well and rely on our rational instincts for the rest.

Term Limits and Legislative Representation. By John M. Carey. New York: Cambridge University Press, 1996. 216p. $59.95.

Michelle M. Taylor, *Texas A&M University*

Term Limits is a wonderful example of the value of comparative political analysis. Textbooks addressing the question "why compare" list making predictions as one reason for the comparative endeavor. Carey's book does exactly that. Through a well-organized, clearly written comparative analysis of the Costa Rican and Venezuelan political systems he creates an empirical basis for predicting what effect term limits could have on politics in the United States.

Using the cases of Costa Rica, which prohibits immediate reelection of deputies, and Venezuela, where there are no restrictions on reelection, Carey examines the effect of "taking away the electoral connection" on the behavior of deputies. A long-accepted premise of political science is that the desire to be reelected drives politicians to attend to constituents' particularistic needs and policy preferences. It is also commonly accepted that, where parties largely determine who gets elected and reelected, parties are cohesive, so they can pass their legislative programs. These virtual axioms of political science and the recent fascination with term limits in the United States prompted Carey to explore the effect of term limits on deputies' behavior. His findings allow prediction that can inform the debate in the United States and also expand theory about the electoral connection and how it influences the ability of democratic systems to represent citizens' interests and make policy.

The book is organized in such a way to make it accessible to many types of readers. Carey reviews the literature about the effect of term limits, then argues eloquently for how a comparative approach will help us assess the effect of term limits on political behavior. He then presents brief but thorough overviews of the Costa Rican, Venezuelan, and U.S. cases. In the second section of the book he conducts a comparative quantitative and qualitative analysis of the effect of term limits, looking at political careers, performance of constituency service, and party cohesiveness. In the third section he brings the discussion back to the U.S. case.

The findings corroborate existing research on constituency service incentives and how parties can control their deputies. First, Carey establishes that Costa Rican deputies have political career aspirations, even though they cannot build a career in the Assembly, and he shows that many do continue in politics through appointed executive branch positions. He then shows how the "electoral connection" need not be direct to induce deputies to attend to constituents' needs. Statistical data and information from elite interviews are used to show that Costa Rican political parties have created an incentive structure that prompts deputies to perform constituency service. In Venezuela, by comparison, deputies do not have an incentive to attend to constituents, even though most want to be reelected. Party leaders determine who gets "electable" positions on the party list, so deputies work for party leaders' interests rather than the people of the country. The Costa Rican findings suggest that patronage is a solution to the collective action problem term limits create for parties. Carey's work adds depth to the constituency service literature. He not only reaffirms existing findings but also fleshes out how the indirect electoral connection works by focusing on politicians' careers immediately after their term in the Assembly.

This work goes beyond the existing literature about the electoral connection in several ways. It provides empirical evidence to validate what deputies say about their constituency service behavior and how it affects their political futures. It shows that even where a reelection incentive is clearly present (as in Venezuela) parties can recast this incentive structure in a way that diverts deputies from constituency service. Carey also provides an excellent discussion of the advantages that can accrue to parties from targeting particularism. This subject is ripe for research as parties, such as the KMT in Taiwan and the PRI in Mexico, make the transition from clientelistic authoritarianism to democracy. Parties, not just individual deputies, may target pork and particularistic benefits in an orchestrated fashion to ensure that they win democratic elections. Carey tackles the difficulties of empirically testing for party strategies that target particularistic resources caused by the multiple strategies which may be used. His combination of statistical tests and information from elite interviews provides insight into how parties attempt to make use of this tool to enhance their election chances.

Carey also explores the effect of term limits on party cohesiveness and the governing party's ability to implement its program. He shows that legislative cohesiveness is lower in the Costa Rican Assembly than in the Venezuelan Congress and that term limits largely explain the difference. Based on this comparative analysis he presents a list of conditions needed for party cohesiveness. These findings have significant implications for the U.S. case, since parties in the United States already lack effective mechanisms to police adoption of the party label. The implication is that term limits could increase party cohesion in the United States if they were to cause deputies to depend on their party to continue their political career. If parties were to continue to exercise little control over politicians' futures, however, then cohesiveness could decrease even more.

The final section of the book returns to the U.S. case. Carey first looks at the behavior of last-term members of Congress to simulate the effect of term limits on the behavior of U.S. politicians. He then considers what the Costa Rica–Venezuela comparison tells us about the possible effects of term limits in the United States.

In sum, Carey uses an innovative comparative method to advance greatly the theoretical and practical debate about term limits. But this book goes beyond that topic and will be of interest to scholars studying party cohesiveness and institutional design. Carey shows, once again, but from a very different angle of research, that there is often a trade-off between responsiveness and party cohesiveness. In addition, this book provides food for thought about how parties can try strategically to target pork and particularistic service to enhance their electoral prospects. This is interesting from the perspective of institutional design, that is, to determine when parties will be able to direct the behavior of their deputies. It also is a potentially rich line of inquiry concerning transitions from authoritarianism to democracy, as parties from authoritarian regimes adapt to competitive elections while trying to ensure their hold on power.

The Green Rainbow: Environmental Groups in Western Europe. By Russell J. Dalton. New Haven, CT: Yale University Press, 1994. 305p. $34.00.

Green Networks: A Structural Analysis of the Italian Environmental Movement. By Mario Diani. Edinburgh: Edinburgh University Press, 1995. 221p. $75.00.

Thomas R. Rochon, *The Claremont Graduate School*

Long before Mancur Olson formulated the illogic of collective action from the perspective of an individual utility maximizer, students of social movements puzzled over the origins and causes of the periodic mobilization of substantial segments of the public on behalf of some collective goal. In recent years, scholars have increasingly moved away from the individual level of analysis and instead have examined the organizations and networks (both formal and informal) that coalesce to lend structure, strategy, and ideological direction to a movement. While it is, in the end, up to individuals to decide whether and how they will become involved in a movement, organizations and networks provide the context for those decisions. In their respective studies, Russell Dalton and Mario Diani have focused on the highly diverse and diffuse networks of organizations that we conventionally group under the name "environmental movement."

The environment is a particularly good issue area in which to study movement structure. The contemporary environmental movement has produced a profound transformation in social values as well as in political institutions and processes. As Dalton points out, there are now ministries of the environment in every European country, the European Union has the Environmental Agency, and environmental interest groups have more members than do political parties. Diani tells a comparable story for Milan, where environmental organizations have won the right to sue local administrators who violate environmental regulations. Most of the organizations studied by Dalton and Diani participate in governmental commissions, giving them a direct (though often modest) role in developing policy.

At the same time, the environmental movement is not a single, homogeneous entity. The contemporary wave of environmentalism is only the latest iteration of a series of movements going back to the late-nineteenth-century surge of interest in preserving natural wonders and exotic species. Each wave of environmental action has produced its own organizations, resulting in a contemporary movement composed of organizations accumulated from different eras like layers of sediment. Each layer has its own distinctive issue agenda, organizational structure, and tactical repertoire.

Russell Dalton's innovative and readable study, *The Green Rainbow*, focuses on environmental interest groups in Western Europe. His survey of 69 national environmental organizations shows that most divide neatly into two types: older groups concerned with nature protection and recently established associations concerned with ecological issues. Dalton reveals that this distinction is highly predictive of a range of organizational characteristics. Relative to nature protection associations, environmental organizations are less centralized, more participatory, and more internally conflictual. Nature protection associations tend to be centered on carving out for themselves a role in the political process; ecological groups also see politics as important, but they are equally likely to view change in public opinion as crucial to achieving their goals.

Dalton concludes that ideology is the key to understanding the structure, resources, and tactics of movement organizations. His model of "Ideologically Structured Action" (ISA) proposes that the ideology of an organization affects its structure, internal decision-making process, resources, tactical choices, alliance opportunities, and specific issue focus. Dalton thinks of environmental organizations as rivals competing in a market, each with its own product differentiated on ideological grounds. Each fills a niche and sells a specific ideological image to members. The societal market for environmental organizations is a segmented one, resulting in diverging organizational types that are able to mobilize a variety of supporters, develop varying relationships with political organizations, and reach alliances with a wide array of social organizations.

Mario Diani's study, *Green Networks*, is in some ways an intensive local version of Dalton's survey of national organizations. Like Dalton, Diani takes as his unit of observation the movement organization, and he surveyed 42 such organizations in Milan in 1985 (along with a smaller set in 1990). Although he interviewed most of the active members in these groups, Diani centered his questions on traits of the organization itself, on links among organizations, and on how the organization recruited its activist base.

Diani's central concern is to elucidate the structure of the network linking environmental organizations. This includes the formal network of interorganizational ties (which Diani calls the visible network) and the informal network of friendships and overlapping memberships among activists (which Diani calls the latent network). These links aid in the circulation of resources and common perspectives throughout the movement; the network structure is in fact the key to creating the sense of shared grievance and common identity that makes it meaningful to speak of a movement in the first place.

Like Dalton, Diani divides environmental organizations into those focused on nature protection (including animal protection and land preservation) and what he calls the "political ecology" movement, which is centered on pollution, traffic, open-land management, and other problems associated particularly with urban growth. Diani finds fewer distinctions between these two organizational types in Milan than does Dalton in his comparison of national organizations. Both branches of the movement grew rapidly during the 1980s, and both were greatly shaped by the student and worker protests of the 1970s. As Tarrow (1989) has pointed out in *Democracy and Disorder*, one effect of that era of protest was to efface the traditional dividing line in Italian politics between red and white, or communist and Catholic. Although Diani finds environmental organizations whose origins can be traced to the Catholic tradition of activism, the Marxist tradition, or the extra-parliamentary tradition that emerged in the late 1960s and 1970s, there are many ties among these organizations, and they are not distinctive in their ideology or their tactics. Cross-organizational cooperation within the environmental movement occurs without regard to the traditional schisms of Italian politics.

Diani's most important findings concern the structure of the environmental network. The visible network (comprised of formal alliances and regular exchange of information among organizations) has a hub-and-spoke design. Larger, more resource-rich organizations take the lead in local campaigns, around which the smaller organizations orient their own activities. Most of the smaller groups have a strong connection to at least one large organization, while links among the smaller organizations are relatively thin. The larger, more bureaucratic organizations also have a relatively dense set of ties with one another, which leads Diani to hypothesize that organizational resources are a more powerful structuring principle than ideological beliefs.

The latent network refers to the ties of friendship and overlapping membership among organizations. This network is much more evenly distributed among organizations, and it extends as well beyond the boundaries of the environmental movement itself to encompass other political groups. A recent body of research has stressed the crossover in values, tactics, and activists among movements, leading to clusters (or cycles) of movement activity. Diani's data on activist friendships and overlapping memberships provide a microfoundation for the theory of movement cycles.

These studies by Dalton and Diani were carried out independently and are not strictly comparable. The national environmental organizations examined by Dalton are, of course, larger and more resource rich than are the local groups studied by Diani. Dalton's organizations range from 1,000 to 100,000 members; the average membership in Diani's organizations is about 500. Whereas 90% of Dalton's organizations had at least one paid staff member, this was true of just 10% of Diani's organizations. Dalton's study covers most major national environmental organizations in all the European Union member states; Diani sacrifices breadth for the ability to examine even the smallest of environmental associations in Milan. Whereas Dalton's survey contains a great deal of information on the contacts of movement organizations with governmental committees and agencies, Diani offers more information on linkages among the environmental organizations themselves.

Neither study tells us much about the political beliefs of environmental activists. If Dalton's ISA hypothesis is correct, then it becomes a pressing matter for future research to determine the extent to which activists select organizational affiliations based on their own ideological beliefs. The case of Italy poses particularly hard questions about the relationship between ideology and activism, for surveys of the Italian public show them to be among the leaders in Europe in pro-environmental attitudes and among the laggards in environmental activity. Diani (p. 182) concedes that even "Italian ecology activists are not particularly keen on practicing ecologically sound life-styles."

That said, the juxtaposition of these studies offers some intriguing contrasts deserving of further research. Among Dalton's national groups and the largest of Diani's local organizations, ideological differences were profoundly important in shaping organizational processes, tactics, and alliances. Among the smaller groups in Milan, ideological differences appear to be sacrificed to the need to develop close ties to other environmental organizations. *The Green Rainbow* portrays national environmental movements as composed of ideologically differentiated organizations, each pursuing its own goals, and each having relatively limited contacts with others. *Green Networks* finds a greater degree of cooperation among local environmental organizations, which must work together to generate sufficient resources to carry out their activities. The key to organizational structuring within a movement, then, may be the interaction between ideology and resources: The more resourceful groups determine the tenor of the movement, while the smaller organizations maintain themselves in specialized ideological niches.

Both Dalton and Diani (particularly Dalton) find that the age of an organization is associated with its ideology, organizational structure, resources, and alliances. There are two possible explanations for this phenomenon. One is a life-cycle explanation: The resource levels and tactical behaviors of the younger ecological organizations will approach those of the older conservation organizations as the former achieve more social and political acceptance. The other possibility is that there are enduring generational differences between organizational types. This would be the case if it were true that organizations develop structures and tactical approaches in conformity to the political climate in which they are founded, and then retain those patterns ever after. The contrast between the establishment politics of the nature protection organizations, rooted in Progressivism in the United States, and the challenging politics of the ecological groups, rooted in the protest era of the 1970s, suggests a permanent generational distinction in organizational structure and style. Of course, only time will tell if this is the case. Through their careful and nuanced studies of environmental organizations, Russell Dalton and Mario Diani have laid a foundation for future research into the structure and evolution of movement politics.

The Power of Politics: New Social Movements in France. By Jan Willem Duyvendak. Boulder, CO: Westview Press, 1995. 251p. $60.00.

Frank L. Wilson, *Purdue University*

A wave of new social movements based on environmental, anti-nuclear, peace, squatters' rights, women's rights, and homosexual rights groups arose and became important political forces in most West European nations during the 1970s and 1980s. They were seen as distinctive from earlier social movements, such as trade unions, regionalist groups, and farm groups, in their goals, organizational styles, emphasis on participant involvement, and nonhierarchical leadership. The new movements introduced new sets of political actors, different forms of participation, new policy agendas, and greater political turbulence in many European countries.

France, however, appeared to many to lag behind in the trend toward growing political influence from new social movements. This was paradoxical given France's long tradition of disorderly politics. In addition, many observers looked at the "events of May 1968" in France as a point of origin for the European wave of new social movements. But, beyond a short-lived antinuclear energy movement in the early 1970s, France appeared much less involved in the broader wave of new social movements that continued for the next two decades in the rest of Western Europe.

Duyvendak finds the explanation for France's lagging new social movements in the "political opportunity structure," a notion developed earlier by Tarrow, Eisinger, and others. The general structure of political institutions, formal and informal procedures of governing, and the configuration of power in the broad sense of the nature and power of political parties and interest groups create this political opportunity structure with which those challenging the status quo must contend. In addition, challengers, such as new social movements, must devise their strategies not only with reference to this political opportunity structure but also with regard to the specific mix of repression, facilitation, and chances of operating successfully within that structure.

As expected, the author focuses on the strength of the French state; he points also to the extensive powers of selective exclusion that the state can use to isolate and demobilize those forces it wishes to ignore; he illustrates how the two-ballot electoral system works to limit opportunities for challengers. Duyvendak also directs attention to a less commonly recognized element in the French opportunity structure: the pervasiveness of traditional conflicts over social class and religion. He argues that the strength to these traditional cleavages and their embedded nature in the party system and in political discourse have worked to limit opportunities for new forces that seek to define themselves outside the parameters of these old cleavages. Indeed, Duyvendak rejects the usual claim that the events of May 1968 represented the birth of the new politics of new social movements. Instead, he argues that the events were "old politics" dominated by class conflict.

In addition, the author looks at the consequences on the new social movements of the arrival of the French Left to power in 1981. This was the first time that the Left had ruled alone since the Popular Front in the 1930s. During decades of opposition, the Left had become the source of political hopes for countless causes, including those of the new social movements. Duyvendak explains how these hopes were shattered, not so much because of the realities of political life but more because of the Left's inability to rise above its concerns linked to traditional cleavages. Duyvendak shows well—and seems to share—the disillusionment of the new forces as Mitterrand's government gave preference to traditional class-based politics and electoral considerations over the aspirations of the new social movements. He sees in this disillusionment the key to explaining the demobilization and demoralization of French social movements.

Two features make this book particularly noteworthy. The first is the author's interest in comparing the French experience in a systematic way to the experience of new social movements in three other European countries: Germany, the Netherlands, and Switzerland. Duyvendak is part of a team including Hanspeter Kriesi, Ruud Koopmans, Marco G. Giugni, and Hein-Anton van der Heijden that has studied new social movements in these countries. The focus on France is enriched by this team's studies of the other three countries. The comparisons are valuable in identifying and explaining the unique features of the French situation.

The second feature is the quantitative analysis. Duyvendak and his colleagues have developed an important data set that allows for the incidence, measurement, and classification of protest events. The data are drawn from the major newspaper in each of the four countries—France, Germany, the Netherlands, and Switzerland. The study includes one issue of the four newspapers from 1975 to the end of 1989. The issues used were selected from Monday editions, which presumably would capture the weekend news, when political demonstrations and other events are most likely to occur. In France, however, the combined Sunday-Monday issue of *Le Monde* actually appears on Saturday afternoon, well before it can report on the weekend's activities. Another potential distortion of the data may come from the fact that many smaller events in France may not all be reported in a country where they are expected as part of the legacy of political contention. On the contrary, in Germany, the Netherlands, and Switzerland, where a tradition of political dissent is not as well established, newspapers may cover events that would be ignored in France.

The book is plagued by minor typographical errors. For example, one source is listed in the bibliography as "D. Haas" but consistently cited in the text as "Paas." There are also a number of minor but annoying factual errors, such as the reference to ENA as the Ecole Normale d'Administration (p. 48) or the assertion that the government, like the president, is directly chosen by the voters (p. 47). The author also has the habit of using italics so very liberally that they lose their ability to emphasize major points; he then resorts to exclamation points in parentheses to draw attention. More important, while there is a bibliography, there is no index, and that is a serious omission in a scholarly work.

Despite these criticisms, the Duyvendak book stands as the best we have on new social movements in France. It also represents one of the few major efforts to look empirically and comparatively at unconventional political behavior since the book by Barnes and others, *Political Action: Mass Participation in Five Western Democracies* (1979). Even with the methodological limitations noted above, *Power of Politics* is an important contribution to our understanding of new social movements in France and elsewhere.

Enterprise and the State in Korea and Taiwan. By Karl J. Fields. Ithaca, NY: Cornell University Press, 1995. 269p. $35.00.

Global Competitiveness and Industrial Growth in Taiwan and the Philippines. By Cheng-Tian Kuo. Pittsburgh, PA: University of Pittsburgh Press, 1995. 267p. $49.95.

Paul W. Kuznets, *Indiana University*

These books, as their title indicates, compare Taiwan with Korea and the Philippines. They have other things in common beside Taiwan: Both examine issues of political economy, both are by political scientists, and both employ an explicitly institutional approach to explain differences in enterprise size (Fields) and in economic performance (Kuo). They differ, however, in their institutionalism. Fields, who writes in the tradition of developmental state theorists, argues that "state institutions . . . are the key determinants of enterprise organization" (p. 23). Kuo discards developmental state theory for a tripartite scheme in which economic outcomes are determined by differences in state-enterprise networks.

Comparison, says Fields, requires that Taiwan and Korea be similar in most important respects but dissimilar in the variable of particular interest. After discussing similarities and disposing of competing explanations, Fields finds that the Korean state, which focused on rapid economic growth, achieved this by heavy subsidy of diversified business groups (the *chaebol*) and chaebol-affiliated general trading companies. In Taiwan the related enterprise groups (*guanxiqiye*) and large trading companies have been much smaller and less influential than their Korean counterparts because a transplanted Nationalist Party regime, though desiring growth, wanted mainly "to promote economic stability and prevent the overconcentration of private capital" (p. 240). Since finance has been the key to state-enterprise relations in each country, Fields examines financial systems and their use by the state to implement industrial policy as well as the institutional differences that have fashioned very different business groups and trading enterprises in the two countries.

Kuo defines a developmental state as one with state autonomy, strong capacity, and committed leadership. He finds that Taiwan, though economically successful, did not fit the definition, while the Philippines under Marcos, an economic failure, did. Success and failure therefore depend on something other than developmental statehood. Kuo's central thesis is that institutions influence economic performance "according to . . . type of institution" (p. 25). He distinguishes three types: laissez-faire, clientelist, and corporatist. The first may or may not aid development, the second produces stagnation, and the third promotes development. Taiwan succeeded because of a shift from clientelism to corporatism, while the Philippines failed, despite the formal trappings of corporatism, because these only masked the replacement of traditional by authoritarian clientelism. Kuo finds support for his institutional thesis in case studies of the textile, plywood, and electronic industries in each country. These, and a concluding chapter, constitute the second half of his book.

Although necessarily brief, these summaries of the two authors' central arguments reveal a host of major issues. One is whether Taiwan has (Field) or has not (Kuo) had the autonomy required of a developmental state. Fields finds the Nationalist state "remarkably autonomous from Taiwanese society" (p. 79), whereas Kuo sees a state "not economically autonomous from the capitalists" (p. 59). Why should autonomy matter? One reason may be the Sunist legacy in Taiwan. Sun Yat-sen was concerned that private profit not be generated at the expense of public welfare and that growth be coupled with equity. Also, the guanxiqiye are dominated by ethnic Taiwanese: Their expansion would threaten the Nationalists' political power. The issues of whether the state was or was not autonomous cannot be settled here, but two points can be made. First, the chaebol have been much larger than the guanxiqiye, both absolutely and relative to GNP, though not as large as Fields shows (Fields's measure, sales as a percentage of GNP, double counts sales of purchased inputs. The appropriate measure is value added, not sales). This supports Fields's argument that the Taiwanese state limited enterprise size. Second, democratization has reduced state autonomy in both countries. Fields may be correct in concluding that "business interests have become dominant players in these new liberal settings" (p. 240).

There is another explanation, which has nothing to do with state institutions, for why enterprise groups and trading companies are larger and therefore more influential in Korea than in Taiwan. This one follows from the scale economies and large minimum-efficient size of capital-intensive industries like steel and automobiles and from differences in domestic market size. Although scale economies may require exporting, domestic markets still must be large enough to support an infant industry before it can compete in world markets. This is why Korea exports automobiles and Taiwan, with only half Korea's population, does not. Also, the combination of large firms and small markets creates monopolies. If these produce steel, petrochemicals, and other intermediate goods, they will reduce the competitiveness of downstream producers. Production of intermediates has therefore been reserved for public enterprises in Taiwan, which is another reason the guanxiqiye are smaller than the chaebol.

Kuo, in *Global Competitiveness and Industrial Growth*, must identify the dominance of a particular institution, determine whether there is economic growth or decline, and then show that growth and institution are related so that corporatism coincides with rapid growth, clientelism with little growth or decline, and laissez-faire with large fluctuations (and either growth or decline). There are three crucial points in this methodology: the identification of the dominant institution, reasons for a shift in institutional dominance, and the causal connection between institution and economic performance. Each of the three poses problems.

Kuo finds, for instance, that the Nationalists imposed state corporatism in Taiwan from 1949, but it was ineffective until the 1960s, when Chang Kai-shek "replaced military recovery of the mainland with economic development as the country's first priority" (p. 67). This is reflected in the textile industry, which languished in the 1950s, then surged until the first oil shock in 1973, after which production stagnated. Kuo examines the rise and fall of corporatist textile associations during these years and concludes that "the growth and decline of the textile industry was associated with its degree of cohesion" (p. 103) and that "a close relationship existed between the state's cooperation and the industry's performance" (p. 104).

These findings reveal methodological problems. First, the presence of corporatism does not mean that it dominates. The issue is not existence but dominance, which is more subject to differences of interpretation than is existence. Second, the change from de jure to de facto corporatism (Kuo's terms) or from clientelism to corporatism followed from realignment of state priorities. A subsequent change from corporatism to laissez-faire in the textile industry occurred when business cooperation eroded and the state turned its attention from textiles to the emerging electronics industry. These explanations for the shift from the dominance of one institution to another are ad hoc and may have no basis in the institutions themselves.

Although Kuo uses such terms as "associated with" and "close relationship" to express the connection between institutions and economic performance, institutions are the causal agents in his analysis. Why, then, should an institution such as state corporatism improve economic performance? Kuo answers that state-producer cooperation will increase linkages from foreign direct investment, improve the use of foreign loans, shield producers from market fluctuations, reduce information costs, improve decision making, and increase effectiveness of policy implementation (pp. 36–43).

One problem with these answers, beside Kuo's failure to provide evidence in point, is that the causal relation may run the other way, from economic performance to institutional type. During Taiwan's textile boom, for instance, cooperation contributed to the state's export goals and the industry's needs for imported machinery and materials. Later, when production stagnated and quotas were established to prevent cutthroat competition, producers and the state had less incentive to cooperate, and so corporatism declined. Also, as with Fields, the economic facts can be explained without reference to the institutional setting. Taiwan's textile boom and subsequent decline were based on exports which rose and fell according to shifts in comparative advantage. Just as low wages allowed Taiwan's textiles to displace Japanese textiles in world markets, later, after wages rose in Taiwan, Taiwanese textiles were displaced in turn by textiles from such lower wage countries as China and Indonesia.

Criticism here should not be allowed to obscure the considerable merits of Fields's and Kuo's work. Disciplinary borders often provide subjects for interesting and significant research, and political economy is no exception. Although institutional matters like enterprise size and state-enterprise networks may play an important role in economic development, the standard methods and theories of economics provide little help in dealing with them. Fields and Kuo take a first step toward identifying, grouping, and analyzing the institutional arrangements that have often been ignored in explanations of economic performance in Taiwan, Korea, and the Philippines. Much of their work is original rather than derivative and should provide new insight for any reader who wants to understand the development process in the three countries.

States and Anti-Nuclear Movements. Edited by Helena Flam. Edinburgh: Edinburgh University Press, 1994. 427p. $85.00.

Alice H. Cooper, *University of Mississippi*

This book studies the interaction between states and antinuclear movements in eight Western European countries in order to illuminate two phenomena: the effect of state-movement interactions on mobilization, goals, and strategies of movements over time, and the effect of state-movement interactions on policy outcomes, shifts in state elites' policy goals, and conflict-management styles over time. The focus is much more on the latter than the former, that is, the political process by which nuclear energy policy is made.

To this end, the book examines a series of "encounters" in successive "arenas" of conflict between state and movement, studying how the succession of strategic moves and countermoves in the context of the broader political system produces the ultimate policy outcomes. This focus provides the framework for the eight case-study chapters, which are of uniformly high quality, are meticulously researched, and provide for very interesting reading.

Theoretically, the book argues that social movements, elite responses, and policy outcomes are much better explained by dynamic, fluid, and contingent processes than by the structural features of the political systems in question. The book rejects the argument, formulated in particular by Herbert Kitschelt ("Political Opportunity Structures and Political Protest: Anti-nuclear Movements in Four Democracies," *British Journal of Political Science* 16 [1986]:57–85), that formal political opportunity structures (e.g., "open" versus "closed" political systems) *alone* account for movement characteristics or policy outcomes, although the authors find that formal structures have some influence on the initial phase of state-movement conflict. Instead, taking a "process-oriented rather than structural" approach (p. 384), Flam (chapters 11 and 12) argues that elite responses to movement challenges as well as policy outcomes are largely a product of "path-defining first encounters" (p. 318), in which movements manage (or fail) to find political allies, bring the conflict into an arena that permits public participation, and so forth.

Similarly, Diani and van der Heijden (chapter 13) maintain that movement characteristics result more from the evolution of encounters and styles of elite response than from "objective" or "stable" political opportunity structures. Finally, Midttun and Rucht argue that highly variable political process explanations must complement structural accounts of nuclear policy outcomes. Neither singly nor collectively do the authors arrive at a theory explaining all phases of all movements. Speaking implicitly for the entire book, Midttun and Rucht note that, contrary to their initial intentions and expectations, "the most generalizable finding in our study, therefore, seems to be that policy outcomes are highly dependent on a specific setting composed of many factors which may, in part, change quickly over time" (p. 410). The contributors do, however, make a series of penetrating, carefully argued, and nuanced observations that bring rigor to the highly varied empirical detail.

States and Anti-Nuclear Movements fits nicely into a body of literature on social movements that explains their characteristics and effect in terms of dynamic political processes. That movements are shaped by their political opportunities is a well-established tenet of the social movement literature. That opportunities can be dynamic or conjunctural, as well as structural and relatively fixed, has also received notice (e.g., David Meyer, "Protest Cycles and Political Process: American Peace Movements in the Nuclear Age," Political Research Quarterly 46 [1993]:451–79). In line with *States and Anti-Nuclear Movements*, moreover, several scholars have incorporated selected aspects of state-movement interaction into broader studies of social movements, including changes in state action as one element of conjuncturally determined opportunities (e.g., Doug McAdam, *Political Process and the Development of Black Insurgency 1930–1970*, 1982; and Anne Costain, *Inviting Women's Rebellion: A Political Process Interpretation of the Women's Movement*, 1992).

This volume makes several useful contributions to the literature. First, compared to most other works, it focuses more explicitly and in more detail on state-movement interaction as an essential structuring element of policy conflicts. Second, while a number of scholars have studied how state structures shape movements and how movements affect political systems, analyses of state actions are rare and generally are less systematic than those found in this volume. States, too, are subject to "political opportunity structures," as it were; they, too, face structural and conjunctural constraints in their dealings with movements. Rather than a one-way path of influence, states and movements shape each other's parameters of action. Third, this book does a masterful job of theorizing the relationship between structural features of a political system and the fluid interaction between states and movements. By analyzing that interaction in terms of "encounters" and "arenas," the various authors determine the relative balance between structural and conjunctural factors at work at any given point in the policy-making process.

Gender and Politics in Contemporary Canada. Edited by François-Pierre Gingras. Toronto: Oxford University Press, 1995. 271p. $28.00.

Linda Trimble, *University of Alberta*

The purpose of this edited collection is to "show a few of the very diverse ways in which gender is related to politics" (p. viii). Gingras does not claim to offer a comprehensive overview of the relationship between gender and politics in contemporary Canada. Instead, the selections are deliberately eclectic. Contributors use a panoply of methodologies, from the purely quantitative to the more normative and/or qualitative, to examine topics as varied as homeless women, electoral reform, equity policy, women in the New Democratic Party, feminism and nationalism in Quebec, and print media portrayals of female politicians. The book's twelve chapters are organized into three sections. The first two parts—"The Party System" and "Public Policy"—are appropriately labeled and relatively coherent but the third, somewhat ambiguously titled "Portraying Gender," combines two chapters on the media treatment of women with two entirely unrelated studies. Indeed, these last two chapters could have been omitted with no ill effect.

This collection provides an important resource to students of women and politics in Canada because several of the chapters are theoretically or methodologically innovative, or address relatively unexplored or underexplored topics, or set the agenda for future research. For instance, in Part I, Jane Arscott contrasts liberal representation theory with contemporary feminist thinking about women and representation. She argues that simply electing more women will not ensure the political representation of women's interests and policy needs, in all their diversity. This conclusion is supported by Manon Tremblay's research on the attitudes of candidates for the 1989 Quebec election. Tremblay designed her survey instrument specifically to address the question of whether or not electing more women will make a difference to political life and public policy. She concludes that women *may* help transform the political system from within, but it depends on *which* women are elected, as female candidates are not united in supporting feminist positions and goals. Tremblay urges further research, especially studies of female behavior within the legislative environment.

The second section of the book is equally valuable. Given the Ontario and Ottawa focus of much of the literature on gender and public policy in Canada, the chapters by Roberta Hamilton and Micheline Dumont, which address feminist organizing in Quebec, are especially welcome. Burt, Jacobs, and Ralston take fresh approaches to the analysis of gender and public policy. For instance, Meredith Ralston illustrates the necessity of giving voice to women, especially the most marginalized, who are invariably left out of and overlooked by public policy discussions. Her chapter documents an alarming disjunction between the perceptions and experiences of an ethnically diverse group of homeless women and the explanations for homelessness offered by neoconservative thinkers and practitioners.

Unfortunately, a few of the contributions are disappointing. There is a significant difference between the chapters which adopt gender as a variable and those which take a feminist standpoint, defined by Ralston in chapter 6 as an approach that "takes women as the main subject of research" (p. 121). The studies which are content to add women typically adapt existing instruments and measures to compare women with men. This approach constructs men as the norm and women as Other. The gender-as-variable strategy falsely homogenizes women, obscuring differences based on ethnicity, class, sexual orientation, spatial location, language, and so on. In contrast, the studies that take gender into account when framing the research question, developing the methodology or survey instrument, and conducting the ensuing analysis offer a richer and more nuanced understanding of the relationship between women and political life.

For example, Gingras's content analysis of Ottawa daily newspapers uses gender as a variable, while Robinson and Saint-Jean use a feminist analysis to investigate print media portrayals of female politicians. The former is purely descriptive while the latter is explanatory. The Gingras chapter confirms that women are rarely mentioned in Canada's daily newspapers, especially in stories about mainstream politics. This result is hardly novel, as the underrepresentation of women in media content is well documented. Furthermore, the reader is left hanging. Which women make it onto the pages of the daily newspaper and why? In what ways are women's social, economic, and political contributions "feminized" or "privatized" by the press? Robinson and Saint-Jean's chapter helps answer these questions. They offer a generational analysis, identifying three cohorts of female politicians and situating media coverage of these women within three distinct discursive frameworks. Moreover, they explore the political implications of media narratives that persistently focus on the sexual relationships, wardrobes, and hairstyles of female politicians, write women out of political life, and portray feminism as a negative personal characteristic.

Another example of the gender-as-variable approach is Archer and Whitehorn's examination of women in the federal New Democratic Party. Women continue to be underrepresented as party delegates and candidates and to some extent as party officials, despite the implementation of an affirmative action policy by the federal party. The authors provide no analysis of this finding beyond the observation that socialist parties are not immune to "male chauvinism" (p. 17). The investigation of issue positions among party activists is equally unsatisfying. Archer and Whitehorn compare attitudes of male and female convention delegates on only a few measures—stereotypical "women's issues" such as abortion, censorship of pornography, and representation of women as candidates and party officials. Yet, the rather striking gender gap they discover on these issues is assumed to be unproblematic for the party because "the male-female divide seems to be greatest on so-called targeted women's issues" (p. 19)! In sum, this chapter compares women with men, tells us how female activists deviate from their male counterparts, and asserts that it does not really matter anyway.

The Gingras chapter misleadingly titled "Gender and the Canadian Military" uses gender as a vehicle to employ a survey of Roman Catholics in the armed forces. This data-driven research project produces inconclusive and potentially misleading conclusions as a result of the narrow and rather homogeneous sample and the ambiguous nature of the questions. We find that similarly situated men and women have very close positions on "motherhood" issues, such as the value of world peace and disarmament. Finally, David Northrup's chapter suggests that the sex of the interviewer may shape survey respondents' answers to questions about affirmative action. This study will undoubtedly prove helpful when developing more effective research tools, but it does not serve the main goal of the book, that is, exploring "some aspects of the relevance of gender to the Canadian political process" (p. viii).

The transformative potential of feminist research discussed by Caroline Andrew in her brief concluding essay is borne out by many of the chapters in this intriguing collection, particularly those which challenge mainstream political science methods, concepts, and theories.

Cabinet Ministers and Parliamentary Government. Edited by Michael Laver and Kenneth A. Shepsle. Cambridge and New York: Cambridge University Press, 1994. 318p. $59.95 cloth, $21.95 paper.

Making and Breaking Governments. By Michael Laver and Kenneth A. Shepsle. Cambridge and New York: Cambridge University Press, 1996. 301p. $59.95 cloth, $18.95 paper.

Paul V. Warwick, *Simon Fraser University*

These two books represent a tandem assault upon the status quo in the study of parliamentary government. Together they define, illustrate, and justify a new theoretical thrust in a field that knows no shortage of complex, untested, and perhaps untestable theoretical constructs. A virtue of this effort is that it is at once eminently testable and refreshingly simple and elegant. If, as the poet said, "Beauty is truth," this is a theory that ought to be true.

The key motivation for the theory is to resolve the dilemma posed by the McKelvey-Schofield "chaos" theorems of spatial voting games. The theorems indicate that, unless one party controls a majority of parliamentary seats, multidimensional policy spaces rarely contain an equilibrium point and therefore should experience extreme instability. The theoretical dilemma is that this expectation is unrealized in actual parliamentary systems, even though most appear to be multidimensional, and many lack majority parties. Laver and Shepsle tackle the problem by assuming that (1) each salient policy dimension falls under the jurisdiction of a single department or ministry and (2) notwithstanding the fundamental principle of collective decision making, government policy in each ministry is determined by the party that supplies the minister. These assumptions imply that the policy of any proposed successor government in each dimension of the policy space must be the policy of the party allocated the ministry governing that dimension. By limiting the policy alternatives that may replace the status quo in this manner, stability becomes much more likely.

In *Making and Breaking Governments*, Laver and Shepsle propose three kinds of equilibria that may emerge under these circumstances. Using Kadane's ("On the Division of the Question," Public Choice 13:47–54) result that an equilibrium point in a continuous policy space must be at the multidimensional median, Laver and Shepsle's first equilibrium is a dimension-by-dimension median (DDM) government. In their terms, such a government would have to include all parties occupying median positions on salient dimensions and allocate to each the corresponding portfolio. If no allowable alternative is preferred by a legislative majority, that is, its "winset" is empty, such a government cannot be beaten. Where a single party is located at the DDM and its winset is empty, we have the special case of a very strong party (VSP). It is in a position to form a stable government by itself (regardless of whether it is majoritarian), since no legislative majority prefers any other alternative. Finally, even if no empty-winset DDM exists, there may be a party so situated that all proposals in its winset must also include it. This gives such a party an advantaged bargaining position, since it can veto all alternatives. Whether it manages to govern alone or not depends on the credibility of its veto threats; it is therefore a "merely strong party" (MSP). But it will be in whatever government emerges, and that government will be in equilibrium.

These types of equilibria hold considerable promise for closing the gap between theory and reality, provided that the key assumption of ministerial autonomy is correct. But is it? The edited volume, *Cabinet Ministers and Parliamentary Government*, sets out to determine its plausibility through examining the policy roles played by cabinet ministers in sixteen parliamentary systems. These qualitative discussions focus on whether the policies that governments adopt are affected by (1) their partisan composition, (2) their allocation of portfolios among parties, and (3) their allocation of portfolios within parties. The first and third questions present few problems and lead to relatively unambiguous answers of "yes" and "no," respectively. The second question, the key one from the viewpoint of the theory, cannot be answered so easily. On the one hand, the decision-making power of individual parties over the portfolios allocated to them is enhanced by the sheer volume of government business, which encourages delegation, by norms of noninterference in the affairs of other departments and, most important, by the fact that ministers typically set the agenda in their domains: They determine which policies are brought forward for cabinet approval and in what form. On the other hand, studies also show that ministers are often constrained by the norms and practices of their department, by the need for interdepartmental cooperation whenever policies cross departmental lines, by the obligation to clear policy proposals with the ministry that controls finances and/or the prime minister, and by the existence of detailed coalition agreements that must be respected.

Most observers would regard the idea that ministers can do whatever they want in their jurisdiction as inherently implausible because it cuts against the very idea of cabinet government. Indeed, ministerial autonomy is rated low in most of the systems examined; on my reading, only Austria and Norway are judged to have even moderate levels, with the Netherlands and Italy falling somewhere in between. At first glance, this would appear to be bad news for their theory, but Laver and Shepsle see it in a less negative light. In their view, the noninterference norm, the agenda-setting power of individual ministries, and their control over policy implementation add up to a strong element of departmentalism. This departmental power may be anticipated in the decision-making processes of cabinets, resulting in decisions that, despite their collective character, reflect the partisan allocation of portfolios. Another possibility, which they do not consider, is that the coalition agreements guiding policymaking in multiparty governments are based on the principle of dividing up policy areas rather than reaching interparty compromises in each of them (perhaps for the same reasons).

The edited volume, although valuable as a compendium of country analyses of ministerial influence, is mute on these points and hence inconclusive on the central assumption of departmentalism. But *Making and Breaking Governments* presents other evidence in favor of the theory. To establish that the strong-party concept may provide part of the answer to the riddle of governmental stability, Laver and Shepsle perform an extensive series of simulations to show that strong parties emerge quite frequently in systems with low dimensionality and a low to moderate number of parties. More important, using data on twelve postwar Western European systems, they amass a considerable body of empirical support for the theory. For one thing, they demonstrate that government formation tends to follow the lines of the theory more often than one would expect by chance, notably with respect to the presence of strong parties in cabinets and the propensity for very strong parties to monopolize key portfolios. Since strong parties tend to be large and centrally located in the policy space, the authors also show that the strong party concept has explanatory power even when size and centrality are controlled.

The case made for the theory has its weaknesses, to be sure. Testing actual formations against random expectations is a fairly easy test, given the size and centrality of strong parties. When measures of size and centrality are entered into the analysis, the results are much more ambiguous. VSPs remain substantially more likely to hold key portfolios, but since they are parties that control all medians, it is difficult to imagine that it could be otherwise. MSPs, in contrast, are no longer prone to receive key ministries while the tendency for their partners (PSPs) to do so is statistically rather shaky ($t = 1.72$). Laver and Shepsle see these results as the consequence of the fact that MSP and PSP status are themselves functions of size and centrality, but if they are the intervening variables in the causal chain, their presence in the model should eliminate the size and centrality variables, not the other way around. No other competing explanations are evaluated, nor is the expectation that equilibrium governments should last longer directly tested.

Laver and Shepsle are aware that more work needs to be done; they have doubts, for example, about another assumption underpinning their theoretical results, the assumption of separable jurisdictions—that the policies ministers implement in their department are unaffected by what other ministers do in theirs. Nevertheless, the authors show that assumptions such as the equal salience of policy dimensions and the attribution of only one dimension to each key portfolio can be relaxed without compromising the results (whether the opposite possibility that departments share policy dimensions can be accommodated is another matter). Moreover, they demonstrate that their theory can encompass such phenomena as the merging and splitting of parties, the willingness of party leaders to allow intraparty factionalism, and the formation of minority and surplus majority (oversized) governments. That so simple a theory can have this explanatory scope makes it powerfully attractive and helps allay—if not dispel—doubts about its central premise.

Peasant and Nation: The Making of Postcolonial Mexico and Peru. By Florencia E. Mallon. Berkeley: University of California Press, 1995. 472p. $55.00.

Martin J. Scurrah, *The Flinders University of South Australia*

Florencia Mallon's book is more than a comparative analysis of rural popular movements in Mexico and Peru during the nineteenth century; it is more than an effort to understand why the attempt to construct a nation-state was relatively more successful in Mexico than in Peru; and it is more than the examination of the articulation of local, regional, and national politics in two countries. Also and most important, it provides us with a new understanding of the ways in which forms of peasant nationalism develop and how subaltern classes actively participate in processes of nation-state formation. It therefore opens up new avenues for researching and understanding the politics of nation-state formation and presents a sophisticated and nuanced vision of the role of popular rural movements in the political debates of Mexico and Peru in the nineteenth and twentieth century.

The starting point for Mallon's research was why peasants and other subordinate groups participated in the political and ideological formulation of national projects without ever forming part of the alliance in power (p. 5). In answering this question through a comparative study of late-nineteenth-century Mexico and Peru, Mallon begins by conceiving of nationalism as "a project for collective identity based on the premise of citizenship—available to all, with individual membership beginning from the assumption of legal equality" (p. 4). She argues that peasant and other popular movements in the nineteenth century represented attempts to gain acceptance as citizens, regardless of age, sex, race, class, or ethnic identity, by promoting a version of nationalism involving a more egalitarian practice.

In the case of Mexico, the Liberals achieved hegemony by at least partially incorporating the political aspirations and discourses of their followers, and this laid the groundwork for the successful consolidation of the nation-state after the 1910 revolution and its culmination under Cardenas in the 1930s. By contrast, the aristocratic republic constructed by the Ceceristas and Pierolistas in Peru excluded and repressed the peasant movements that arose in resistance to the Chilean invaders during the Pacific War, thus impeding the achievement of hegemony and the consolidation of the nation-state, as evidenced by the rise of Shining Path in the 1980s and the formulation of new nationalist projects.

In making this argument Mallon does not oversimplify or romanticize the peasant popular movements she studies but carefully documents the actions, struggles, conflicts, and alliances involved in the process of achieving communal hegemony out of the multiple discourses of gender, race, ethnicity, and class. She identifies local intellectuals as key actors in both formulating the communal hegemonic discourse and articulating this with discourses at the national level. These local intellectuals were "those who labored to reproduce and rearticulate local history and memory, to connect community discourses about local identity to constantly shifting patterns of power, solidarity, and consensus" (p. 12). In doing so, they had to choose between acting as local counterhegemonic heroes or enforcers of more dominant forms of political culture. By the late twentieth century, according to Mallon, local intellectuals largely played the second role in Mexico and the former in Peru.

After an introductory chapter, in which the origins and basic outline of the argument are presented, the book is divided into three sections. The first presents a detailed description and analysis of the process of achieving communal hegemony in the indigenous communities in the Sierra Norte de Puebla in Mexico during the Liberal Revolution of 1850–1867 and of the relationships between these alternative nationalisms and hegemonic discourses. The second section widens the analysis to incorporate less detailed information from three other cases: Morelos in Mexico and Junín and Cajamarca in Peru. These cases serve to test the generalizability of Mallon's argument by discussing instances in which the articulation of local and national nationalist projects was unsuccessful or in which there was a failed attempt to achieve communal hegemony, and they also sensitize the reader to many of the factors conditioning these complex and dynamic processes. Finally, in the third section, the cases are compared and contrasted in detail, conclusions and reflections are presented, and the similarities and differences are carefully laid out between Mallon's work and that of such researchers as Womack, Jean Meyer, Guerra, Knight, Hale, and Korek, Nugent, and Alonso, in the case of Mexico, and Flores Galindo, Manrique, and Gootenberg, in the case of Peru.

Because *Peasant and Nation* presents a new approach to researching and understanding the process of nation-state formation in the nineteenth century, it is of interest and relevance to historians and political scientists who are not specialists on Mexico, Peru, or Latin America in general. The book combines complex and sophisticated theoretical arguments concerning political and historical processes with detailed scholarly research in two regions in each country.

In addition to the general argument, the political scientist reader will enjoy the discussion of local intellectuals and the employment of the bones found in a cupboard in the museum in Xochiapulco as a metaphor in a long and subtle reflection on local versus national hegemonic discourses. Yet, this reviewer found the detailed description of events in the Mexican case studies to be difficult reading in comparison to the briefer descriptions of the two Peruvian regions, with which he is more familiar. Thus, the reader who is not a Latin Americanist may wish to concentrate on the introductory chapter and third section of the book.

The consolidation of the nation-state in Mexico and the failure to achieve this in Peru are, of course, the result of processes involving many more regions and a longer historical period than those covered in this book. Thus, it is a suggestive rather than a definitive analysis of the process of nation-state formation in each country. One also wonders whether the peasant uprisings in Chiapas and Guerrero (the latter encompassing the area of one of Mallon's case studies) and the present difficulties confronting the country may call into question Mallon's characterization of the Mexican nation-state as "consolidated." Nevertheless, this challenging and insightful work is recommended to all political scientists interested in the processes by which popular and elite and local and national politics are articulated.

Poverty, Inequality, and the Future of Social Policy: Western States in the New World Order. Edited by Katherine McFate, Roger Lawson, and William Julius Wilson. New York: Russell Sage Foundation, 1995. 723p. $70.00.

Desmond King, *St John's College, University of Oxford*

Advanced industrial democracies are experiencing significant economic and social changes. Not only is the economic basis of many of these states undergoing restructuring away from manufacturing toward service-sector employment patterns but also, combined with demographic and social trends, the welfare regimes in these countries face immense challenges. *Poverty, Inequality, and the Future of Social Policy* is an ambitious and valuable contribution to the analysis of these trends. In a series of high-quality essays, the contributors range over the fundamental changes to household structure, labor markets, poverty levels, and welfare programs of selected advanced democracies (Canada, France, Germany, Italy, the Netherlands, Sweden, the United Kingdom, and the United States).

In the context of rising unemployment—particularly among the young, ethnic minorities, and middle-aged workers—and changing occupational structure—an increase in part-time, short-term contract employment—Western democracies have been compelled to concentrate upon mechanisms and institutional arrangements able to assist the transition from either welfare or unemployment into the labor market. This problematic is at the core of the essays in this book. Intellectually, these developments coincide with the emasculation of the social-democratic welfare state consensus and the success of various versions of the New Right neoliberal framework. The elements of the former are valuably identified by Guy Standing in his chapter on the labor market: labor market security, employment security, work security, job security, labor reproductive security, labor representation security, and income security. Standing then documents how, for the weakest members of the job force, these dimensions of labor market participation have been eroded in many industrial democracies. Standing introduces one of the major themes of the collection: the incipient shift to a "behavioral conditionality" approach in work-welfare characterized by tough entitlement regimes. He argues that in the new tougher labor market environment, unemployment benefit regimes are transformed into "primarily regulatory devices [which] only partially protect the unemployed, since even if benefits are paid, income protection is far from secure" (p. 182). Not only has job security declined, but also income support increasingly is awarded conditionally, for example, through workfare or job-search schemes.

In an excellent chapter Katherine McFate correctly identifies the public employment system as the key mechanism through which the state intervenes and attempts to ameliorate conditions in its national labor market. If government programs designed to improve labor market conditions and income levels are to be effective, then a reliable employment service is essential. Yet, as she recognizes, this is exactly an area in which industrial democracies have mostly failed (a conclusion consistent with my own research published in *Actively Seeking Work? The Politics of Unemployment and Welfare Policy in the United States and Britain*, 1995). She summarizes the institution's importance lucidly: "The public employment service of a country is the command center of active labor market policies. A national or regional system for identifying job shortages, emerging skill demands, and labor surpluses must be in place if a government is to effectively place citizens in activities that have the potential to improve their employment prospects. Unfortunately, few of the nations without developed capacities in these areas invested in upgrading their public employment services in the 1980s" (p. 643). Mostly, public employment services become institutions for monitoring claimants, more attentive to their conditional activities than to helping them find jobs. Only Sweden, Italy, and France, for example, require employers to notify vacancies to employment offices; in other countries, these services deal with jobseekers at the margins of the labor market. This bias is very problematic because, as McFate writes, employment services must be focused on the needs of unskilled and often young or minority jobseekers, for whom jobs are hardest to find.

In the United States, the uniqueness of African Americans' historical experience—denial of equality of citizenship until the early 1970s and persistent economic disadvantage in the preceding decades—continues to be a reason for federal policy. These circumstances are the focus of Troy Duster's impressive chapter. Black Americans, particularly young males in central cities, face immense problems in integrating into the labor market, as the legacies of the egregious and pernicious segregated race relations in the United States remain tenaciously present. Duster identifies the major recent trend: "an aggregation of people, mainly African American but increasingly Latino, who live in the central cities, participate at unparalleled levels in both alternative and underground economies, and have extraordinarily high rates of youth unemployment and contacts with the criminal justice system" (p. 466). As McFate observes, there has been a "dramatic decline in demand for low-skilled labor . . . over the last decade and a half in Western democracies" (p. 631).

Furthermore, Duster rightly reminds the reader that the U.S. perception of African Americans' current difficulties is remarkably ahistorical and ignorant of the segregationist past: "Our cultural memory is so short that there is a tendency to regard this very recent development as endemic and a long-standing feature of American society" (p. 466). His most dramatic data concern the alarmingly common encounters with the prison and criminal systems experienced by young African Americans, worryingly formulated in the following statistic: "While there are more than 10 times as many white males in higher education than incarcerated (4,485,000 vs. 330,258), there are more black males in prison or jail than attending colleges and universities on a full-time basis (341,622 vs. 270,301)" (p. 474). The scale of these problems for U.S. society could hardly be expressed more pellucidly.

This carefully compiled and useful collection of essays has toward its conclusion a magisterial discussion of social rights by Hugh Heclo, a scholar whose work on social policy regimes has been influential. Heclo, to summarize all too briefly, argues that the development of welfare policy regimes in twentieth-century democracies has had an inclusionary bias, a feature he suggests is seriously threatened by recent labor market exclusions and welfare debates. Without pursuing policies which include all citizens, however, our industrial democracies will observe the evaporation of their civil bases, the restoration of which may prove a good deal harder than many of us suppose.

Civil Society and State Relations in Sweden. By Michele Micheletti. Aldershot: Avebury, 1995. 200p. $59.95.

Henry Milner, *Queen's University*

Michele Micheletti does three things in this short book. She begins with an introduction to the study of collective action organizations and their evolution, offers an account of their historical development in Sweden in the last 150 years, and concludes with an attack on the standard approach in the existing Swedish literature. The first task is carried out in the initial chapter, the best one of the book. The second task takes up the bulk of the text, the historical analysis colored by the author's implicit normative critique, and a critique is rendered explicit in the concluding chapter.

Micheletti begins with civil society, the sphere of organized activity independent of government. A well-developed civil society is the sine qua non of democracy. Collective action organizations are pivotal: They "help rethink democracy, redefine democracy, and change the boundaries of the political" (p. 6). Chapter 1 presents a number of useful typologies, especially those depicting the life-cycle of collective action organizations. The author's implicit normative position begins to emerge when she addresses theories of pluralism and corporatism. Pluralist organizations are "governed more by member values and needs"; they are tolerant of dissent and encourage expression of minority viewpoints. Corporatist organizations, in contrast, are "rigid . . . characterized by elite democracy" (p. 15). Turning to "political opportunity structures," Micheletti moves quickly from ideal-type typologies to the real world: that of the United States is pluralist, open to all kinds of organizations, while that of Sweden is corporatist "because only certain collective action organizations are allowed regular and routine access to government" (p. 26).

Over the next six chapters Micheletti offers an historical account of the evolution of Swedish collective-action organizations dating from the "formation of modern civil society" in the latter nineteenth century. She identifies five historical periods in the twentieth century: democratization, from 1900 to 1920; the emergence of the "strong society" in the 1930s; the "Swedish model" of the 1940s and 1950s; dissatisfaction in the 1960s and 1970s; and "the repluralization and reemergence of Swedish civil society of the 1980s and 1990s." Much useful information about the waning and waxing of the fortunes of organizations in the context of wider Swedish developments is provided, although it would have helped greatly to set these developments into comparative context to allow the reader to distinguish what is unique about Swedish experience.

The account is skewed by Micheletti's normative perspective. In effect, the present "repluralization and reemergence of Swedish civil society" is the standard against which previous periods are measured. For example, in discussing the early period, Micheletti argues that apart from the classic threesome comprising the free church, temperance, and trade-union movements, "two other social movements . . . deserve a place in this Swedish hall of fame. They are the women's and peace movements" (p. 39). These are strange choices for the classical "hall of fame" compared, say, to the consumer cooperatives. Indeed, Micheletti admits as much when describing them as going into "abeyance" until the postwar years.

When we arrive at the most recent period, the author stresses the rise of groups expressing "post-material" values, such as environmental, women's, and international aid organizations, and the decline of the most powerful groups of the heyday of the Swedish model—especially the trade unions. Her implicit normative orientation is unmistakable: The former deserve to gain at the expense of the latter. One byproduct of this emphasis is that insufficient attention is paid to the third and most important type of voluntary organization in Sweden. This type is set up primarily to provide services to a societal constituency, including organizations concerned with sports, popular culture, various hobbies, adult or consumer education, and, in recent years especially, the needs of retired persons.

In the concluding chapter Micheletti makes explicit her position by contrasting it to that of others. The development of "new identities" by which Swedes define themselves as "individuals and concerned citizens" to replace the old collective identities is a good thing, since the traditional interest organizations, and especially the main trade-union confederation, LO, "evolved into political correctness" in the form of elitism, universalism, and conformism (pp. 175–6). The author's harshest language is saved for (unnamed) scholars "closing their minds—and therefore ours" to the flaws in these organizations and the corporatist structures and social-democratic hegemony which bolstered their position (p. 181).

It is hard to assess the validity of this critique from the substantive material provided in the book, which is much more about the relationship between organizations (civil society) and the state than about individuals and organizations. Apart from references to the ten graphs in the appendix, which trace membership levels for the major organizations, the members are very much in the background. And Micheletti's main critique, that dependency on the (welfare) state rendered these organizations elitist and conformist, is not supported by figures allowing for any overall comparative or longitudinal analysis. Indeed, what we know suggests the opposite. For example, researchers at Stockholm's Skondal Institute, who are participating in a ten-nation study based in England, and those working on a 13-nation study based at Johns Hopkins University, with the aim of comparing voluntary organizations, report that Swedish organizations continue to be comparatively more successful at financing their activities through membership dues and are less dependent on public financing than are groups elsewhere.

Most troubling, in my view, is the treatment of the dimension of public discussion. While Micheletti refers frequently to public discussions around such fundamental questions as co-optation of the organizations by the state, she does not give the organizations she targets credit for fostering those discussions. Indeed, she turns the argument around, for example, attacking as hypocritical labor-oriented critics of TV advertising campaigns mounted by employers' organizations to bypass the usual avenues of public discussion (p. 152). Though the effort to educate the public is noted with regard to the temperance associations, free churches, and adult education societies, it is absent from the discussion of the trade unions and allied popular organizations. In this, Micheletti is being no less one-sided than the targets of her critique. She ignores the long-standing involvement of trade unions in popular education, in furthering literacy and reading, and, more profoundly, in contributing to a sense of self-worth and pride of accomplishment among Swedish workers, which came from knowing that one's representatives were respected and their inputs taken seriously.

Especially in the absence of a comparative context, the sharp criticisms in the book's final pages leave the reader wondering: compared to what? to a past or future Swedish "Golden Age"? to what the members of the trade unions want? Uninformed readers no doubt would be surprised to learn that the unionized proportion of the Swedish work force remains the highest in the world—and still without recourse to closed-shop legislation.

The author's intended meaning is not always apparent from the words as they appear on the page. The book is an abridged version of one first published in Swedish. Clearly, inadequate attention has been paid to recasting the material for the non-Swedish reader. This is exacerbated by stilted writing and the absence of copy editing. It takes an act of will to keep reading a book when regularly confronted with sentences like that on page 2: "In the fourth period is called rethinking politics and the dissatisfied generation (1960s–1970s)." Yet, the reader able to exercise such will, and discount the gratuitous attacks, will not go unrewarded.

World Military Leaders: A Comparative and Collective Analysis. By Mostafa Rejai and Kay Phillips. Westport, CT: Praeger Publishers, 1996. 164p. $55.00.

Thomas A. Baylis, *University of Texas at San Antonio*

An enduring question of leadership studies is whether certain individuals possess personality traits that would permit them to emerge as leaders in very different occupations and social contexts, or whether leadership is largely situation specific, that is, whether the qualities that make a man or woman successful as a leader in one setting will also do so in quite different circumstances. The tantalizing noncandidacy of General Colin Powell for the U.S. presidency recalls the frequency with which successful military leaders worldwide have sought to move into the political arena, either on the crest of a wave of popular acclaim or through the perhaps more common self-recruitment mechanism of a coup d'état. For political scientists, the question of whether the qualities that lead to military distinction are similar to those required for political success thus holds a particular fascination.

To be sure, Mostafa Rejai, a political scientist, and Kay Phillips, a sociologist, do not directly address this issue in their book on world military leaders. Indeed, they explicitly deny that their book is about military *leadership* at all. Their purpose is more modest: to present a "comparative, collective portrait" of high-ranking military officers, focusing on the sociodemographic, psychological, and situational factors that led them to the military profession. The authors offer what they call an "interactional theory" that treats the relationship among these factors as responsible for the rise of military leaders. Such a study could, however, also be suggestive of insights into the relationship between military and political leadership, the more so since Rejai and Phillips themselves have written a series of books that seek to analyze different types of political leaders.

The approach the authors have chosen is to analyze the biographical information available on 45 renowned military leaders from 13 countries who lived between the 1600s and the 1980s. As the authors recognize, this "sample" is anything but random: More than half are American or British, and only five are from outside Europe or the United States. Twenty-five are primarily twentieth-century figures. Moreover, all are far from being typical military leaders; they are celebrated figures in their country's history. Since no control group of comparatively unsung leaders is examined, the applicability of the study's findings to "military leaders" in general is questionable.

Even if we put aside these qualifications, the authors' findings are unspectacular. As a rule, the military leaders studied are mature men when they reach their highest rank, belong to mainstream ethnic and religious groups, come from military or professional families, and have conservative and nationalistic views. Nearly half experienced relative economic deprivation and somewhat more than one-third underwent "love deprivation" (meaning in most cases the early loss of one or both parents). Slightly more than one-third were in some sense—socially, culturally, physically, or psychologically—"marginal." Nearly half were "particularly noted for their vanity" (pp. 130–1). The authors were somewhat surprised to find that ten members of their sample were inclined toward religiosity, mysticism, or superstition and that ten were "mathematically inclined."

These findings would be of greater interest if they contrasted markedly with the attributes of leaders in other fields. But in a three-page chapter entitled "Seeking Comparative Patterns," the authors reveal that their effort to use factor and discriminant analysis to compare their military sample with the "revolutionary" and "loyalist" leaders they had investigated earlier failed to yield significant results. "Accordingly," they rather disarmingly conclude the chapter, "we dispense with the presentation of our 'findings' " (p. 33).

The reader is left with a series of brief biographical "profiles," one or two pages in length, of the 45 leaders, concentrating on their childhood and the first part of their career. These accounts occupy the larger part of a rather slender volume. In some cases they offer suggestive glimpses of one or another aspect of the leaders' development; for the most part they emphasize the obstacles faced, self-doubts, and character flaws more than achievements. Perhaps the public successes of military leaders, like those of Lasswell's politicians, are the products of efforts to compensate for unsatisfied personal needs. But the profiles, based in most cases on a small number of biographies and occasionally autobiographies, are too sketchy to be entirely persuasive.

The study suffers, overall, from its tendency to fall between two stools: It is not quite an elite study comparable to Janowitz's classic *Professional Soldier*, nor is it a leadership study. The sample is too small and too unrepresentative to lend itself to compelling statistical analysis but too large to permit an intensive examination of individual cases. Studies of elites and of leaders invariably make methodological compromises, but one wonders in this case whether the insights the authors present are adequate compensation for such compromises.

Agrarian Reform in the Philippines: Democratic Transitions and Redistributive Reform. By Jeffrey M. Riedinger. Stanford: Stanford University Press, 1995. 366p. $45.00.

Mark R. Thompson, *University of Glasgow*

Debates about the "dilemmas of simultaneity"—dual transitions to democracy and the market—should not obscure another key issue that has often accompanied regime change: demands for social reform. The end of authoritarianism in many countries has raised hopes not only of more political openness and higher living standards but also of greater socioeconomic equality. In few recent transitions have expectations of social change been greater than in the Philippines after the dictatorship of Ferdinand E. Marcos collapsed in February 1986. His corrupt rule had worsened inequality generally and land distribution in particular, despite pledges to build a new society. A major communist challenge arose as reform faltered. When a peaceful, mass-based uprising brought Corazon C. Aquino to power before the communists could topple Marcos by force, there was a widespread belief that she would continue "people power" and marginalize the revolutionaries by carrying out comprehensive agrarian reform.

Jeffrey Riedinger has written an important study of agrarian reform in the Philippines that adds substantially to our knowledge, despite James Putzel's earlier path-breaking analysis, *A Captive Land: The Politics of Agrarian Reform in the Philippines* (1992). Riedinger's book is a product of fieldwork in the Philippines for nearly a decade, beginning almost immediately after Aquino's rise to power. Although Riedinger modestly omits references to his own role, he was, as a colleague of Roy Prosterman, a major contributor to the early debates about agrarian reform in the post-Marcos Philippines. As a scholar, he has sifted through a mountain of material in a systematic and fair manner. He also conducted interviews with many of the key participants. The extensive footnotes are often as interesting as the main text.

As readers familiar with Philippine politics already know, the tale is depressing. Yet, Riedinger resists the temptation to let disappointment at the Aquino government's unwillingness and inability to carry out extensive land redistribution color his analysis. He even shows that, despite the toothlessness of the agrarian reform legislation passed during Aquino's presidency, some progress was actually made by implementing the old Marcos program, primarily on rice and corn lands. In fact, Riedinger demonstrates that *more* reform has been undertaken by Aquino and her successor, Fidel V. Ramos, than by all previous Philippine governments taken together. But given a very low base, this has still not led to a significant reduction in endemic landlessness in the country.

Riedinger's theoretical framework focuses on the chances of agrarian reform under different regime types. He asks whether agrarian reform is more or less likely to occur during democratic or authoritarian rule (although he muddles this distinction somewhat by claiming that Philippine democracy is "exclusionary"). Rather than offer quantitative comparative data on regime type and agrarian reform, he makes reference to several crucial cases—in particular, El Salvador, Japan, Kerala State in India, South Korea, and Taiwan (but not the interesting example of revolutionary Portugal)—to show that reform can be carried out under either authoritarianism or democracy. In addition, he draws lessons from recent Philippine history. Under Marcos's authoritarian rule, "reverse land reform" was probably greater than any actual redistribution. This leads Riedinger to conclude that authoritarianism is neither a necessary nor a sufficient condition for reform. But, as shown by the pre- and post-Marcos Philippines, democratic governments may not do much better.

Yet, this (rather fragile) generalization does not tell us much about the importance of democratic *transitions*, as the book's title promises. While many categorizations can be made, the ends of the spectrum are fairly clear: On the one hand, there have been negotiated transitions between regime and opposition elites, on the other, insurrectionary democratizations in which the ancien regime has collapsed in the face of massive popular protest. It has been theorized that negotiated transitions are unlikely to yield much social change because of agreements providing guarantees to key elite actors. "People power" in the Philippines, however, is very close to the insurrectionary type. It may be thought that such a transition is more likely to create conditions favorable to social reform as the old guard has been replaced without being able to extract any guarantees. Such an analysis would help us better understand why conditions for reform appeared so favorable after the fall of Marcos.

Riedinger rightly points to Aquino's class background (she is the scion of a major landowning family, and her brother, an influential congressman, was at the forefront of the landlord lobby) to explain her reluctance to carry out agrarian redistribution. She refused to decree a comprehensive program before the new Philippine Congress convened, instead issuing only a vaguely worded executive order urging reform. But it must be asked why Aquino was the overwhelming choice to lead the moderate opposition to Marcos in the first place (even among leading social reformers!). During the presidential campaign against Marcos, agrarian redistribution was not heavily stressed by Aquino. Riedinger's chapter, "Campaign Promises," overemphasizes the two pro-land reform speeches she did make. Rather, the opposition concentrated on a cross-class "crusade" to topple Marcos, and Aquino, as the wife of a matyred politician and as a supposedly nonpolitical "simple housewife," seemed the ideal person to lead it.

Even if Aquino had been more committed to agrarian redistribution (and, in fact, after the January 1987 massacre of peasant demonstrators, a cabinet committee was established that considered land reform), it must be asked whether it could have been undertaken in the face of repeated military threats to her government. Riedinger documents these dangers but does not adequately assess their effect on the chances of reform. Had Aquino been perceived as a reformer like Aristide in Haiti, might she, too, not have been toppled by conservatives in the armed forces? Aquino (barely) clung to power by compromising with the Ramos faction of the military, launching an antiinsurgency campaign and firing several reformist cabinet ministers.

Riedinger's analysis of Aquino's refusal to implement reform by fiat is problematic in another sense as well. Would such an extensive use of executive power have been any less authoritarian because it served a "just cause"? Aquino's opponents had already accused her of ruling arbitrarily after she abolished the "Marcos Constitution" and handpicked delegates to the Constitutional Commission, granting herself virtually unlimited authority in the meantime. Although the new constitution was overwhelmingly ratified, the congressional elections that followed shortly thereafter in May 1987 were a crucial part of the government's claim to being democratic. To preempt the legislature on such a major social and political issue would have weakened this legitimacy and made it unclear whether reform was a product of dictatorship or democracy.

Not surprisingly, the landlord-dominated Philippine Congress passed legislation that was riddled with exceptions and delays. Riedinger examines why a powerful left-of-center political party which could have pushed substantial agrarian reform through the legislature never emerged. Although lobbying efforts by nongovernment organizations linked to the communists and the social democrats with the support of the reformist wing of the Catholic Church were unprecedented, they failed to have much effect on a congress in which land reform had very few friends. Riedinger, rephrasing James Scott, speaks of "everyday forms of resistance" by the elite. But his argument comes down to a claim about the persistence of clientelism that creates cross-cutting cleavages which undermine peasant solidarity. While this is a common theme of studies about the Philippines, Riedinger's analysis is very general and offers little fresh insight into the slippery terrain of "Philippine culture."

Riedinger also does not make clear enough the likely consequences of the failure of agrarian reform in the Philippines. He warns of growing peasant unrest but does not explain why the communist New People's Army is currently in steep decline despite the lack of meaningful land redistribution. Other factors, such as democratic regime change, have apparently contributed to the weakening of the insurgency. Under Ramos, the Philippine economy has begun to improve. Agrarian reform has been pushed off the political agenda. This suggests that while Philippine society is unlikely to become fairer in the near future, it may soon become more prosperous.

Founding Republics in France and America: A Study in Constitutional Governance. By John A. Rohr. Lawrence: University of Kansas Press, 1995. 370p. $45.00 cloth, $19.95 paper.

Jeremy Rabkin, *Cornell University*

This is an engaging and rewarding book because it takes an unusual approach to its subject. It is also a frustrating book for the same reason. The book is essentially a survey of constitutional norms in the French Fifth Republic, presented with an eye to "what might illuminate American constitutionalism and administration" for "an audience primarily of American students, practitioners and professors of public administration" (pp. xiii, xii). Rohr gives some attention to particular defining moments in the constitutional history of the Fifth Republic and also looks at a number of rulings by the Constitutional Council and various judicial authorities in France. He also intersperses his account with brief forays into U.S. constitutional history to point up particular parallels or contrasts.

Most of Rohr's attention, however, is focused on the "founding documents" of the Fifth Republic—the various position papers, reports, and records of the government officials who drafted the 1958 Constitution, together with the comments of the outside scholars they consulted and the criticism and promptings they received from a special parliamentary advisory committee. Rohr seems to have achieved considerable mastery over this body of material. He presents its highlights in a clear, engaging exposition in the course of surveying, in successive chapters, the arguments over the major institutions in the new system. Nothing of this sort—at least on this scale—is otherwise available in English, and it is by far the most valuable contribution of this work.

The controlled, bureaucratic process that produced the text of the Fifth Republic Constitution may seem far removed from the free-ranging discussions at the Philadelphia Convention or the lively pamphlet war that accompanied the ratification debates in eighteenth-century America. But, in fact, Rohr shows that there are many parallel themes. Much like the U.S. framers, the founders of the Fifth Republic set themselves against a tradition associating legislative authority with the voice of the people. As in the United States, the Fifth Republic founders sought to counter this tradition of legislative supremacy by providing an independent constitutional status to judicial and executive offices. A chapter entitled "Publius and the Gaullists," which compares leading themes in *The Federalist* with the arguments of the Fifth Republic founders, turns out to be quite intriguing and suggestive. Rohr, for example, finds Michel Debré, de Gaulle's minister of justice, celebrating personal ambition in rather Hamiltonian terms. Even more candidly than Hamilton, Debré lauds attraction to "power for its own sake" as a necessary ingredient of success in executive office and a wholesome antidote to the petty preoccupations of parliamentary politics (p. 201). So, too, *The Federalist*'s disdain for "parchment barriers" is echoed (inadvertently, it seems) in the French deliberations by rhetorical snorts against *murs de papier* and *barrières de papier. The Federalist*, however, infers from the inadequacy of legal admonitions the need for countervailing institutions, while the French drafters draw the conclusion that one must simply repose more trust and confidence in the executive (pp. 196–7).

For all its undeniable fascination, Rohr's focus on the official or formal arguments in the latest French founding has its drawbacks. The problem is that in elevating the "issues" in the official debate to a high level of abstraction, one gets a rather artificially narrowed perspective on what was really at stake—even in relation to long-term practice. For better or worse, this problem is inseparable from Rohr's method and quite apparent in his earlier works on the constitutional status of U.S. administration. So, for example, in one of his most influential works, *To Run a Constitution* (University of Kansas Press, 1986), he conjures with highly abstract formulations of the concerns of the U.S. founders to justify the modern administrative system: "the higher reaches of the career civil service fulfill the constitutional design of the framers by performing the balancing function originally assigned to the Senate and the career civil service *en masse* heals the defect [as the anti-Federalists saw it] of inadequate representation [in the House of Representatives]" (p. 171). The current work offers no such firm conclusions, but its subtext—that there is much for Americans to admire in the current constitutional setting of French administration—invites skepticism in much the same way.

Take, for example, Rohr's repeated emphasis on the Fifth Republic's departure from the tradition of absolute parliamentary sovereignty, a tradition he traces (with other scholars) to the influence of Rousseau (p. 130). But Rousseau's *Social Contract* is very clear that to preserve the generality of the general will, the sovereign people must not interfere in any way with the particular implementation duties of the "government" (that is, in U.S. terms, the executive). It has long been noted in France itself that popular sovereignty in principle is too readily reconciled in practice with a rather authoritarian administrative system (with arrogant officials purporting to be "only the docile instruments of this pretended [general] will," as Benjamin Constant protested in the early nineteenth century). French administration has long been renowned—or notorious—for its autonomous discipline, even under the Third and Fourth republics. Does the new system of separated powers, then, make ordinary administration more accountable to politicians?

Surprisingly, Rohr gives no systematic attention to this obvious and important question. He does tell us, however, that the new constitutional ban on members of parliament holding administrative office was considered, from the outset, as making it more likely that high civil servants, rather than career politicians, would be appointed to ministerial posts, smoothing the way to a "technocratic" government (p. 105). Rohr also describes in some detail the curious notion of the Fifth Republic framers that the president should be an ultimate, nonpartisan "arbitrator" of the state (so that, for example, it struck the framers as perfectly acceptable for the president to preside over the judicial council as well as the cabinet). Such hints suggest that far older traditions have survived into the Fifth Republic, traditions that are not at all well captured by Rohr's penchant for rephrasing them in terms familiar to the U.S. constitutional tradition.

In the concluding chapter, Rohr salutes the Council of State "as a concrete refutation of the facile assumption that vigorous administration is somehow at war with individual rights" (p. 254). But his account of the council's work is so abstract that he does not even bother to defend the council's institutional merging—seemingly so at odds with U.S. principles—of judicial authority and an active governmental advisory role. Most readers will doubt that Rohr's invitation to "Franco-American dialogue on constitutional principles" can be fruitfully conducted at this level of abstraction. But Rohr certainly offers many illustrations of Franco-American differences, amid the many parallels, that are well worth the attention of U.S. scholars.

Israel and Its Bible: A Political Analysis. By Ira Sharkansky. New York: Garland Publishing, 1996. 313p. $45.00.

Paul R. Abramson, *Michigan State University*

Ira Sharkansky follows the tradition of such political scientists as Steven A. Brams, Daniel J. Elazar, Michael Walzer, and Aaron B. Wildavsky to study the Hebrew Bible to draw political insights from a book filled with wisdom, inspiration, and ambiguities. He also draws upon his own previous book (*Ancient and Modern Israel*, 1991), in which some of his arguments were originally formulated. Although *Israel and Its Bible* is filled with complex and often brilliant arguments, its basic theses are simple. First, "the political culture of modern Israel reflects certain traits of politics apparent in the Bible" (p. 5). Second, "the character of Judaism draws on biblical traits . . . and Judaism adds its own explanation to politics in the Jewish state" (p. 5). As I will conclude, the first point is somewhat problematic. The second is undoubtedly true. No one can understand modern Israeli politics without understanding the Hebrew Bible.

Sharkansky divides his book into three parts. First, he discusses the political significance of the Bible, noting that he will focus on its political relevance rather than provide a comprehensive analysis of its meaning. Second, Sharkansky analyzes three major political leaders—Moses, David, and Jeremiah—as well as the Book of Job and Ecclesiastes. Lastly, Sharkansky directly analyzes the modern relevance of biblical politics.

Sharkansky analyzes the Hebrew Bible from the perspective of a secular Jew with a deep understanding of the Bible, Jewish traditions, and Israeli politics. His understanding of Hebrew allows him to study the original text, thereby avoiding misleading English-language translations. He is highly skeptical about the reality of events reported in the early Bible, even arguing that there is no nonbiblical evidence for the existence of David (let alone for Moses and the patriarchs). Even scholars who disagree with his skepticism will find a great deal of wisdom in his analysis of Moses, and he clearly points to problems with Wildavsky's interpretation (see *The Nursing Father*, 1984) of Moses as a political leader, correctly arguing that Wildavsky finds more pattern to Moses' political leadership than the biblical text warrants.

As Sharkansky correctly observes, David "is the Bible's most sharply defined political figure" (p. 101), and his discussion of David is particularly insightful. David leads a rebellion against Saul, rules Judah from his capital in Hebron for seven years, and then, with the conquest of Jerusalem from the Jebusites, rules a united Judah and Israel for 33 years. Although Judah and Israel divided under David's grandson, Rehoboam, the dynasty established by David lasted for 400 years. But despite David's greatness as a political leader, he also committed adultery with Bathsheba and facilitated the death of her husband, Uriah the Hittite. Moreover, although he was able to rule over a united Kingdom, he seemed incapable of ruling over his own family, allowing the incestuous rape of his daughter Tamar by his son (Tamar's half-brother) Amnon to go unpunished, faced rebellion by his sons, and failed to assure his succession, requiring Bathsheba to intercede for Solomon to prevent a potential civil war. David, as Sharkansky points out, was not an absolute despot, even in the prime of his early kingship, for he was openly chastised by the prophet Nathan for his adultery and was punished for his transgressions. Sharkansky emphasizes that he is referring to the biblical not the historical David (for whom he finds no evidence) but concludes that "the reality of the young David may have been closer to a bandit chief than a saint, but such an emphasis would not do for the founder of a dynasty that lasted for 400 years and whose seed was to provide a messiah" (p. 119).

Sharkansky also provides a fascinating analysis of Jeremiah, who is doubtless the most political of the prophets, and here Sharkansky finds more historical evidence to support the biblical record. His discussions of the books of Job and Ecclesiastes are interesting, for these are difficult texts to interpret. Clearly, the Book of Job can scarcely encourage trust in God's behavior to man, but Sharkansky wisely avoids discussing God's action (or inaction) during the Holocaust.

Although I found the first two sections of Sharkansky's book enlightening, its importance for political scientists seeking to understand Israeli politics rests on the final three chapters. It is here that the author must support his two central theses: that the political culture in Israel reflects some traits apparent in the Bible and that modern Israeli politics cannot be understood without understanding the Bible.

Sharkansky's first thesis is that modern Israel is guided by "bounded principles" found in the politics of the Hebrew Bible. The politics of ancient Israel was not democratic, even to the extent that Greek city states were democratic, but the Bible describes assemblies as necessary to ratify covenants and reveals that even kings were limited by prophets. As Sharkansky notes, the Book of Deuteronomy sets limits on Israelite kings. He points to modern Israeli institutions that limit state authority, discussing the role of the state comptroller, and several highly critical reports by appointed commissions. He further argues that the principle of "coping" with problems, rather than trying to solve them, is a characteristic of both ancient and modern Israel. Obviously, there are parallels for these modern institutions and practices in ancient Israel, but there are differences as well. After all, modern Israel is a parliamentary democracy, with free competitive elections, a secret ballot, and a list system of proportional representation with a 1.5 percent threshold that guarantees a wide variety of opinion in the Knesset. Sharkansky may err on the side of emphasizing parallels between ancient and modern Israel and deemphasizing differences.

But Sharkansky makes a convincing case that modern Israeli politics cannot be understood without an understanding of the Hebrew Bible, especially since a large minority of Israelis view the Bible as the revealed word of God. Even though the early Zionists were secular, they recognized that a Jewish state had to be created in the Promised Land. The special problem of Jerusalem, which Sharkansky discusses from the standpoint of municipal government, cannot be understood without recognizing the special significance accorded the City of David in the Hebrew Bible. Nor can one understand the claim that Jews have a right to live anywhere within Eretz Israel (the Land of Israel) without appreciating the belief of many Israelis that the holiest Jewish sites are in the West Bank.

Sharkansky begins his book with a postscript written after the assassination of Yitzhak Rabin. I have the benefit of writing my review after the 1996 Israeli election. As Sharkansky noted, even after the 1992 elections, the Orthodox parties had a large presence in the Knesset, and their representation grew from 16 of the 120 seats in the 13th Knesset to 23 in the 14th Knesset. Moreover, the Orthodox vote went overwhelmingly to Benjamin Netanyahu. Why, one may ask, did Orthodox, and especially ultra-Orthodox, voters support a candidate who was a confessed adulterer and whose favorite restaurant, Fink's, is the most prominent nonkosher restaurant in West Jerusalem? Basically, they did so because a Netanyahu victory assured that Meretz, a highly secular list to the left of Labor, would be excluded from power. And from the Orthodox standpoint, Shimon Peres was as much a transgressor of Halakhah (Jewish Law) as Netanyahu. True, he was not a self-confessed adulterer, but his disapproval of David's behavior "from the rooftops" (see II Samuel 11:2) during a Knesset debate in December 1994, an allusion to David's adultery with Bathsheba, may have provoked more animosity among the ultra-Orthodox than Netanyahu's acknowledged acts of adultery (for which he expressed contrition). Without making too much of this single incident, one fact is clear: One cannot understand Israeli politics without understanding the Hebrew Bible. Sharkansky's discussion sheds a great deal of light on both subjects.

Industrial Policy, Technology, and International Bargaining: Designing Nuclear Industries in Argentina and Brazil. By Etel Solingen. Stanford: Stanford University Press, 1995. 311p. $47.50.

David J. Myers, *Pennsylvania State University*

Nuclear energy programs in Brazil and Argentina have attracted worldwide attention since the late 1960s. Etel Solingen's book examines these programs in the context of explaining the sources of policy differences, discussing them as a special case in the quest for "high technologies" by newly industrializing countries. Specifically, Solingen wants to know why, in defining their nuclear industries, Argentina emphasized the participation of its national private firms, whereas Brazil favored state-owned enterprises and joint ventures with foreign firms. Concern is with the choices of nuclear industrial structure and technology, rather than with why states seek nuclear energy in the first place. Solingen also examines consequences flowing from the choice of alternative policies to develop and master nuclear technology and from the initial decision to acquire a nuclear power industry. Examining the Brazilian and Argentinean nuclear programs from these perspectives is an innovative departure from previous research that focused on their strategic implications.

This volume, an extensive revision of the author's dissertation, employs cases of most similar design and uses five different historical conjunctures in Brazil and Argentina, thus broadening the number of observations where arguments can be tested. Solingen favors the institutionalist paradigm, although she assesses its predictive power in relation to such competing explanations as political economy, interest group theory, and ideology. Institutionalism emerges from her comparisons as the most powerful predictor of choices for nuclear industrial structure and technology. The cases used in this research skillfully integrate social science theory, technical materials, newspaper accounts, and interviews with policymakers in both countries. While a more systematic recounting of the number and kinds of individuals interviewed would have increased our understanding of the data underpinning the author's assessments, Solingen clearly gained access to a broad range of important nuclear industry policymakers in Brazil and Argentina. Her work is methodologically rigorous, a significant accomplishment in light of the sensitive and confidential material being assessed.

Two important findings emerge from this research. First, it demonstrates why certain sectoral arrangements take shape and not others. The explanation resides in differing degrees of macropolitical consensus, on the one hand, and bureaucratic autonomy, on the other. Macropolitical goals are defined as "those at the apex of the state's hierarchy of goals and means" (p. 10). Second, this study finds the moment of institutional consolidation to be critical in determining the effect of macropolitical consensus and sectoral autonomy. During Brazil's crafting of its nuclear power industry the government exhibited a more or less consensual hierarchy of goals and means, and state bureaucracies enjoyed slight autonomy. Thus, Brazilian nuclear policy followed the officially sanctioned industrial model: rapid (export-oriented) growth and macroeconomic stability. State entrepreneurship and foreign technology became the means toward that end, leaving relatively little room for national private industrial and technological resources. Argentina, in contrast, exhibited a low macropolitical consensus. The tripartite division of the state among the armed services strengthened the autonomy of each service within its industrial sphere of influence. This situation broadened the services' range of options, allowing the preferences of the Navy-controlled Atomic Energy Commission to prevail. These preferences led to the commission's emphasis on domestic private entrepreneurial and technical resources.

The author argues persuasively that structure influences the selection of high industrial technologies and their outcomes, but she is less compelling when she contends that her study demonstrates the superiority of institutionalism over the interest group, political economy, and ideology approaches. Indeed, this research is not designed as a test of the predictive power of disciplinary paradigms. Were this its purpose, it would have to examine several additional contenders to institutionalism: rational choice, political culture, and elites.

Industrial Policy makes several important contributions to existing scholarship. First, it expands the literature on the nuclear sector, largely limited to the industrial world, by probing the experience of nuclear industries in two industrializing countries. The author presents this experience as a sectoral example of the more encompassing quest for high technologies. In this context Solingen provides new perspectives and information about how entrepreneurial activity mirrors levels of macropolitical consensus and bureaucratic autonomy and how it can influence a sector's market. In Brazil and Argentina the nuclear industry market reflected state intervention and mercantilistic objectives, not only in regulatory activities and reactor choice but also in the emphasis on nationalization of inputs and in the underwriting of research and development in fuel-cycle and reactor designs.

This volume also adds to our understanding of how politics and economics interact when industrial countries acquire high technology and the consequences of this interaction. In the domestic arena Solingen documents that the causal effects between economic and technological structures, on the one hand, and sociopolitical configurations, on the other, flow in both directions. Thus, political and economic environments direct inputs into the industrial sectors, but the outcome of that intervention affects the legitimacy and longevity of ruling coalitions as well as the particular consensus they bring to bear on policy matters. From the perspective of global resource movement, this study is an antidote to unidirectional dependency theorizing. Solingen carefully documents how the use and structuring of domestic political institutions influence the terms under which foreign suppliers transfer capital and technology. She also implies that how foreign suppliers configure themselves influences their bargaining positions in such transactions. To summarize, this research confirms that institutions do matter. In remains unclear, however, whether they matter enough to dictate different outcomes in the quest to industrialize.

Finally, Solingen identifies activities that merit the attention of other social scientists when she moves beyond nuclear programs to explore the applicability of her conceptual framework for other sectors of the Brazilian and Argentinean economies. She demonstrates that her study's basic model explains choices of industrial structure and technology in other sectors, as well as outcomes, and argues persuasively for its relevance to other industrializing countries and for sectors in advanced industrial economies. This suggests that taking the next logical step of probing cross-industrial differences in light of variations in macropolitical consensus and bureaucratic autonomy could advance significantly our understanding of the political power of private actors, the process of international bargaining, and the determinants of technological and political change.

Parties and Politics in Post-1989 Poland. By Hubert Tworzecki. Boulder, CO: Westview Press, 1996. 219p. $54.00.

Paul J. Best, *Southern Connecticut State University*

Anyone trying to follow the post-1989 development of parties and politics in Poland from North America using the English-language press would be completely lost. Even using a North American Polish-language newspaper such as New York's excellent *Nowy dziennik* (News Daily), what could one make of the existence of extreme right- and left-wing political parties in Poland and even the existence of a party called the Friends of Beer. I suspect that even many in Poland cannot make head or tail of the apparent chaos. It looks like the old canards are true—for every two Poles there are three political parties, or Poles are unable to govern themselves.

Into the breech steps Hubert Tworzecki with his study of Polish politics from the restoration of Solidarity to legitimacy in spring 1989 through the Polish presidential elections of 1995, which pitted the Nobel Peace Prize winner Lech Walesa against the "repainted communist"/socialist Aleksander Kwasniewski. It is the goal of Tworzecki to prove that the chaos is more apparent than real and that Polish politics do not deviate all that much from the European norm, something he does admirably well. How does he do it?

The book has 219 printed pages, but these are not all text. In an unusual fashion, the front matter ends on p. xvi, and the main text begins on p. 17. By my count, there are 19 pages of notes, 64 pages of figures and tables, a 16-page bibliography, an index of four pages, an appendix of two pages, and a one-page endnote. These, with the 16 introductory pages, total 122 pages of support for 97 pages of straight narrative text. The author in this somewhat overkill fashion wants to be "in sharp contrast with earlier scholarship, which saw post-communist electoral behavior as mysterious, inchoate, and unpredictable." He wishes to show "that even in the early stages of democratization, electoral behavior is based on systematic quantifiable factors."

What Tworzecki did was marshal all the tools of modern political analysis to show that "choices made by Polish voters were shaped by regional, demographic, and attitudinal factors that have remained remarkably consistent from one election to the next." He does this by means of a complete literature review and by analyses, in the form of tables and figures, of all the data he was able to gather.

Tworzecki's seven-page conclusion compares the Polish political situation to that of Western Europe. He concludes that there are both similarities and differences. Certainly, middle-class, working-class, and peasant-based parties developed all over Europe in the nineteenth century, including partitioned Poland. In resurrected Poland after 1918 the parties of the three former partition zones had difficulty in forming nationwide alliances. The post–World War II situation, however, froze or prevented political activity since the Soviet-supported Communist nomenklatura dominated politics. After 1989 it appeared that Poland had " 'skipped' the era of mass parties and moved directly to weak partisanship, skeptical electorates and conditional support," which seem to be part of Western Europe today. The reason for this situation, however, is not a "skip" but, rather, no history of successful long-term coalition-building politics in East Central Europe. The author concludes that Polish voters nonetheless have been selecting from a "consistent set of political alternatives: market- and social-liberal, social-democratic/socialist, anti-communist/religious-traditionalist and populist." Postcommunist Polish politics are not chaotic but, rather, serve as a solid foundation for a new party system for Poland.

In summation, Tworzecki's book is a solidly researched and more than solidly supported study of the current situation in Poland. It certainly cleared up many misconceptions that this 30-year observer of the Polish scene had developed. This book can be recommended without reservation to libraries and to scholars interested not only in Poland but also East Central Europe and postcommunist developments.

Judicial Misconduct: A Cross-National Comparison. By Mary L. Volcansek, with Maria Elisabetta de Franciscis and Jacqueline Lucienne Lafon. Gainesville: University Press of Florida, 1996. 163p. $39.95.

Kenneth M. Holland, *University of Memphis*

As the case of Mexico indicates, a reputation for corruption and impropriety diminishes the authority of the courts and fosters public cynicism concerning the administration of justice. As the history of the U.S. Supreme Court testifies, policymaking from the bench can lead to popular outrage and cries of judicial tyranny. This volume addresses both problems. How do democratic nations ensure that judges perform their official duties in a proper and effective manner, thereby earning the public's respect? How do democracies ensure that judges do not make social policies that harm the public interest or are unacceptable to a majority of the citizens? An obvious solution to both challenges is to deter potentially errant judges by punishing those who are corrupt, incompetent, or tyrannical. The central finding of this substantial contribution to the comparative study of courts, however, is that judicial discipline poses enormous problems for Western democracies. In fact, they punish very few judges for unethical, uncivil, or unprofessional behavior. The reason is not that judges in these regimes avoid misfeasance and malfeasance but that sanctioning magistrates threatens principles highly valued by popular governments, including judicial independence, the rule of law, and security for the rights of minorities. In particular, all regimes that secure private property resist policies which threaten the rule of law, a principle requiring insulation of the judiciary from political influence.

The authors point out that the problem of personal misconduct by magistrates exists in all democratic regimes but that the second problem, lawmaking by politically unaccountable judges, is peculiar to the United States. The scope of the study is a description of the history and current methods of disciplining errant judges in France, Italy, England, and the United States. The two chapters on judges in France and Italy are written by indigenous scholars. An introductory chapter lays out the dilemma posed by unaccountable judicial power in democracies, and a concluding chapter summarizes the preceding national studies, drawing some valuable cross-national comparisons.

This is the first attempt to provide a thorough overview of the way in which these four democratic polities tackle the problem of judicial misconduct. The historical perspective provides much needed context for discussions of current practice and likely future developments. U.S. readers will find the comparative dimension helpful in highlighting the ways in which the state and federal judiciaries operate differently than their counterparts across the Atlantic. A main conclusion of the comparison, namely, that diversity and innovation in judicial discipline are much greater in the American states than in any of the other jurisdictions studied, supports those who defend the states as laboratories of policy experimentation.

According to Volcansek, the nature of judicial accountability shifts according to the regime served by the judge—authoritarian, predemocratic, or democratic. In authoritarian regimes, judges are subject to removal and punishment, including death, if their behavior or decisions offend the sovereign. In predemocratic polities, such as monarchal and Fascist Italy, judges are civil servants, largely autonomous but with a strong expectation of loyalty to the state. Democratic societies demand that judges be accountable to the people, through such mechanisms as legislative impeachment, popular election, and recall.

Judicial accountability is a more severe problem in the United States, since judges in England, France, and Italy do not have the power to declare acts of the legislature unconstitutional (with the lone exception of the justices of the Italian Constitutional Court). There are, therefore, more ways in which to remove U.S. judges. No English high court judge has been removed since 1830. Only a handful of French and Italian judges have paid the ultimate penalty—loss of office—since World War II. Seven federal judges in the United States have been removed by impeachment since 1789. By comparison, between 1985 and 1995, almost 200 state judges were removed from the bench. This contrast between the state and federal courts is ironic, of course, because the most egregious judicial lawmaking occurs at the federal, not state, level. Efforts to make federal judges more accountable, including passage of the Judicial Conduct and Disability Act of 1980, which allows for complaints against a judge to be filed with a committee of other federal judges, are addressed to personal misconduct, not political behavior.

A somewhat surprising finding is that English judges, who among the four sets of magistrates are the least likely to question governmental authority, enjoy the highest public esteem and best record of commendable conduct. The aristocratic character of the English high court judges, appointed by the Lord Chancellor from among the country's most distinguished senior barristers, with no regard to political affiliation, and paid handsome salaries, seems to ensure public respect. An intriguing revelation is the French principle of judicial reserve, according to which judges, as civil servants, are expected not to criticize governmental leaders or their policies, behavior for which they can be sanctioned.

The central question animating the book—how to solve democracy's dilemma of politically unaccountable judges who have reached beyond merely interpreting the law to making it—tends to lose its relevance during most of the narrative. English, French, and Italian judges are constrained by the reality of parliamentary sovereignty and the lack of judicial review. There is also a problem of comparing apples and oranges—the Italian magistracy, for example, includes both judges and prosecutors, and separation of powers is only observed in the United States. Nevertheless, students of comparative judicial politics will find this volume a welcome contribution to the scanty judicial discipline literature.

International Relations

An Agenda for Peace 1995. By Boutros Boutros-Ghali. 2d ed. New York: United Nations, 1995. 159p. $7.50.

An Agenda for Development 1995. By Boutros Boutros-Ghali. New York: United Nations, 1995. 132p. $7.50.

An Agenda for Democratization. By Boutros Boutros-Ghali. New York: United Nations, 1996. 56p. $5.00.

Bruce Russett, *Yale University*

The first two documents reprint reports initially issued in 1992 and 1994, respectively, with the subsequent addition of related materials such as Boutros Boutros-Ghali's important *Supplement to an Agenda for Peace* published in January 1995. The final document, on democratization, was released in the last days of 1996, when he was a lame duck secretary-general. Forming a trilogy, they set forth his holistic vision of how the UN can and should contribute to a stable and peaceful international order, one in which democracy, development, and peace form an interdependent, mutually reinforcing set of relationships.

Boutros-Ghali is an intellectual, with an earlier career as professor of international law. Consequently, he takes ideas seriously, is interested in theory, and reads. Moreover, he conceives the position of secretary-general as uniquely situated for articulating an international vision and providing intellectual and moral leadership to the global community. These reports, formed in the confrontation of theory with practice, are far more stimulating and direct than one might expect from a leader with such diverse constituencies. A wide range of political scientists should take his ideas seriously, as he has taken theirs seriously. In addition, his vision has been expressed through (and despite) an intensely political process that itself deserves understanding.

All three volumes take as their departure the end of the cold war and the need for transitions in thinking and policy. *An Agenda for Peace* emerged at the height of hopes, fueled by the successful enforcement action against Iraq, for a new role for the UN. The new tasks required a clearer and expanded intellectual foundation for the explosion of UN activity in the realm of security, which Boutros-Ghali provides by distinguishing conceptually among preventive diplomacy, peacemaking, "traditional" peacekeeping, and postconflict peace-building. In addition, he sets out the concept of peace enforcement, not against outright aggression but as a "provisional" measure to restore and maintain a cease-fire more vigorously than peacekeeping forces are enabled to do. In practice and with shifting mandates (as in Somalia and Bosnia), this last concept proved analytically and operationally murky. These experiences blunted the secretary-general's calls elsewhere in the document for greater capacities for the UN and greater contributions from its member states, distracting attention from two themes he would stress in later documents: the importance of democracy for peace within and between nations, and the application of democratic principles within the UN itself.

The *Supplement,* a "reinterpretation in the light of the experience," retreats from the initial call for a rapid deployment force permanently available to the Security Council, to a "strategic reserve" provided by member states. It reemphasizes the need for consent of the parties in peacekeeping and tries to restore the previously blurred boundary between mandates for peacekeeping and enforcement. At the same time, in implicit response to critics, Boutros-Ghali stresses the need for unity of command, for member states to provide clear mandates to the organization and to follow up on their commitments, and for postconflict peace building.

Although flawed, *Agenda for Peace* is a major analytical statement, and it attracted great attention. Neither can really be said for *An Agenda for Development.* Boutros-Ghali and his advisers are not necessarily to blame for this. In the developed nations, at least, security has always held far greater public and elite attention than has development. Development theory has largely given way to Friedmanite economics, and the global North seems ever more resistant to the calls for redistribution of wealth that still often emanate from the South. Political and professional divisions over the theory and practice of economic development have become debilitating, exacerbated in international relations in ways that outlasted many of the cold war security debates. Divisions within the UN "family," as between the contrasting doctrines informing the UN Development Programme and the Bretton Woods institutions, illustrate the problem.

A cacaphony of suggestions, in a highly politicized atmosphere, required a process that extended through more than 40 drafts. Reflecting this, Boutros-Ghali states early in the *Agenda* that it "should not attempt to provide a new macroeconomic theory." But the lack of any clear intellectual center deprives the document of much of its potential bite. He identifies but cannot resolve the tension between recognizing that "governments can no longer be assumed to be paramount economic agents," yet they must intervene where necessary, retaining "the responsibility to provide a regulatory framework for the effective operation of a competitive market system" with the hope of "finding the right blend" of government direction of the economy and encouragement of private initiative. Development requires an agent to ensure some equality of opportunity and basic well-being.

Consequently, much of the document's utility lies simply in its detailed recounting of the activities in development by myriad parts of the UN, with a call for greater coordination of the system. But it does not answer questions of how units with such diverse mandates and constituencies can be coordinated, and whether tight coordination—given, as the author notes, key differences in perspective between the industrial G-7 and the developing Group of 77—would really be all that desirable. Political tensions are also apparent in the document where he endorses "democracy as good governance" (the latter an emergent World Bank principle), discusses the "natural connection between democracy and development," and calls democracy "the only reliable means of improved governance"—but notes that democracy in practice is far from perfect even in developed countries. His linkage of democracy and development is nonetheless evident, as are his convictions that peace is the foundation of development and, more fundamentally than expressed in *Agenda for Peace,* that the deeper foundations of peace lie in spheres of the economy, society, and environmental protection.

An *Agenda for Democratization* is Boutros-Ghali's valedictory, a full expression and synthesis of his "conviction that peace, development, and democracy are inextricably linked." He recognized all along that his commitment to democracy would be toughly contested, and he held preparation of this document tightly in his own office. Completed in something like its final form a year before its publication, it lay suspended while the secretary-general campaigned for reelection. Some of his natural allies on the principle of promoting democracy (notably, the U.S. government) gave him no points for it. Authoritarian states in Asia and elsewhere, and their intellectual allies in Europe, vigorously resisted its expression. They told him that democracy was a Western concept and that his endorsement of it was a betrayal of his own cultural roots. The document is, therefore, less forceful than it might have been. Still, it is a notable statement representing intellectual integrity and courage.

The tensions are evident. It begins by identifying resistance to democracy, which "in some cases seeks to cloak authoritarianism in claims of cultural differences," yet asserts "the undeniable fact that there is no one model of democratization or democracy suitable to all societies." "It is not for the United Nations to offer a model. . . . " Consequently, Boutros-Ghali avoids giving what political scientists would recognize as an operational definition of democracy, emphasizing rather (as in the title) democratization as "a process which leads to a more open, more participatory society." Nonetheless, most of the elements of our professional definitions of democracy are there: "All individuals have a right to take part in the government," "periodic and genuine elections," "power changes hands through popular suffrage rather than intimidation or force," "opponents and minorities have a right to express their views," "an institutional balance between the State and civil society," and respect for "the rule of law."

Boutros-Ghali asserts that "a consensus on democracy and its practical importance has begun to take shape." In response to those who say democratization is alien to the mission of the UN, he roots it powerfully in the Charter, the Universal Declaration of Human Rights, and, with deft touches, in the Declaration on the Granting of Independence to Colonial Peoples and resolutions of the General Assembly. Chapter 4 constitutes a lengthy report on the wide range of activities undertaken—almost by stealth—by UN institutions and programs to facilitate democratization, without forcing states "to apply external models or borrow extraneous forms of government."

The author directly addresses the question of whether democracy and development are complementary or competing goals, and he rejects the claim that they are in conflict. His formulation conforms with what most American social scientists believe: "Although development can take place without democracy, there is no evidence that the breakthrough to development requires an authoritarian regime." Furthermore, "over the long run, democracy is an ingredient for both sustainable development and lasting peace," and "a culture of democracy marked by communication, dialogue and openness . . . helps to foster a culture of development." Nevertheless, he evades "an easy answer to the questions of prioritization and timing," with reference to the inapplicability of any one model to all societies, and he asserts, in an unfortunate phrase, that "each State must be free to determine for itself its priorities for the welfare of its people." This use of "State" is rooted in UN-speak and means member government, here reflecting the need for the UN to respect the sovereignty of its members; nonetheless, many of us would prefer the term "each society."

Despite scattered qualifications, Boutros-Ghali's message about the need for democracy within countries comes through clearly. Sometimes the transition from comparative to international politics works effectively, as in a paragraph which accurately summarizes what is known in our trade as the "democratic peace" principle. But clarity is lacking in chapter 5, "Democratization at the International Level." More than in the case of democratization within states, clearness is compromised by the unwillingness to define democracy precisely. Dimensions of equality, participation, representation, and power are mixed together. His motivations plausibly include coopting those states which, while denying the democratic principle of one-person-one-vote at home, demand application of the one-state-one-vote principle in the UN. Another motivation surely derives from frustration with the United States, demanding control but unwilling to pay its bills, expressed with reference to "States possessing vast power [which] refrain from international involvement commensurate with their strength." "Dominance by one country or a group of countries must over time evolve into a democratic international system." Yet another is doubtless a conviction that "unrepresentative decisions on global issues can run counter to democratization within a State and undermine a people's commitment to it. Thus democratization within States may fail to take root unless democratization extends to the international arena." This is reasonable—but should powerful states which are manifestly undemocratic at home, and thus not properly representative of their people, expect to benefit from democratic principles in the UN?

Boutros-Ghali recognizes that democracy cannot mean the same thing within states and at the global level. His proposed solution includes strengthening various nonstate actors, including nongovernmental organizations, regional organizations, and "civil society" (with paragraphs on business, media, and even academia). This much is familiar to analysts of democracy within countries. But the fundamental difference, in Boutros-Ghali's own words, is that "there is no international structure equivalent to that of State government." Democracy works within a state—even a multicultural one—in which there is a higher level of consensus on both process and goal than exists globally. It also works because there *is* a state which can enforce decisions reached by a legitimate process and can compel contributions by those who would free ride.

Little of this exists at the international level. Arguably the most "democratic" institution in the UN is one of its weakest, the General Assembly, often derided as an ineffectual talk-shop with little power to enforce its resolutions passed on the one-state-one-vote principle. The strongest institution in the UN system, when it can reach agreement, is the manifestly elitist Security Council. The council has some power precisely because it has been controlled by most of the states which have great power in the international system. Would a more democratic UN system be more powerful, or less? I also see no acknowledgment (which would offend many readers) that the ideology of democracy advanced so persuasively in this volume is internationally powerful precisely because it has been propounded by a hegemonic state and coalition.

Boutros-Ghali's vision of a peaceful international system is not entirely in focus. Inevitably, it does not escape some basic contradictions of theory and of practice. Yet, most of the right elements of the picture are there. Internationally as well as nationally, institutions must be seen as legitimate, not just as immediately effective. In the long run, effectiveness depends on legitimacy. Democracy is an instrument for achieving both. Boutros-Ghali's documents should be read with a scholar's critical eye but with appreciation also. He used his privileged "bully-pulpit" well.

Red Flag over Hong Kong. By Bruce Bueno de Mesquita, David Newman, and Alvin Rabushka. Chatham, NJ: Chatham House Publishers, 1996. 189p. $25.00 cloth, $17.95 paper.

A Legislature Comes of Age: Hong Kong's Search for Influence and Stability. By Kathleen Cheek-Milby. Hong Kong: Oxford University Press, 1995. 317p. $55.00.

Hong Kong, 1997: The Politics of Transition. By Enbao Wang. Boulder, CO: Lynne Rienner Publishers, 1995. 231p. $45.00.

Steve Chan, *University of Colorado*

Sometimes described as the "miracle on the rock," Hong Kong's economy has performed admirably during the past five decades. With a small area, large population, and few natural resources, this British colony has displayed tremendous economic adaptability and vitality. As noted in *Red Flag over Hong Kong* (p. 25), its accomplishments include the world's busiest container port, sixth highest household purchasing power, and eighth largest trading entity in merchandise value. With a per capita income exceeding $25,000, it is also among the world's top ten countries according to this measure of affluence—ranking it above Great Britain (p. 11).

This economic achievement has until recently coexisted with political stagnation. In contrast to most of the contemporary world, where the principles of democratic governance and self-determination are at least accorded rhetorical deference if not observed in actual practice, Hong Kong's British rulers did not bother themselves with either for about 150 years. As noted by Kathleen Cheek-Milby (p. xi), "up to 1985 its colonial government, a nineteenth-century relic, had a royal governor, appointed by Britain, who possessed such pervasive power that he appointed all of the members of his executive and legislative councils, controlled their agenda, and if need be, ignored their advice." Questions about Hong Kong's autonomy, stability, and democratic prospects and the political rights of its people have gained increased attention only recently, a phenomenon more due to concerns over impending political developments than beliefs about abiding political principles.

On July 1, 1997, Hong Kong will cease to be a British colony. China will recover its sovereignty over this territory, which it lost to the British after a series of military and diplomatic setbacks in the nineteenth century (beginning with the infamous Opium War, 1839–1842). According to the Joint Declaration signed by China and Britain in December 1984, Hong Kong will become a special administrative region of China. Except for foreign and military affairs, Beijing pledged to allow Hong Kong a high degree of local autonomy. For instance, it will be able to keep its separate currency, its status as a free port, its financial independence (including in taxation), and its participation in international fora. As a gesture of reassurance, Beijing further declared that it will maintain Hong Kong's capitalist system and its current way of life for at least fifty years after 1997.

How credible is China's promise? How much autonomy will Hong Kong actually enjoy after reversion to Chinese jurisdiction? What implications does Chinese sovereignty hold for Hong Kong's future? These are themes shared by the three books reviewed here.

Bueno de Mesquita, Newman, and Rabushka are the most pessimistic. They see Hong Kong's future being tied to the political competition in China, which is likely to suffer increased turmoil and strife in the power struggles to succeed Deng Xiaoping. According to them, even if Beijing's leaders originally intended to honor their pledge, the dynamics of China's domestic politics eventually will produce policy outcomes that will undermine Hong Kong's "free press, academic freedom, open and fair election, and some portion of market freedom.... The political and economic landscape will be filled with uncertainty, cronyism, lost freedoms, and more corruption than has been known in the recent past" (p. 119). Indeed, they offer not only a bleak picture for Hong Kong but also for China. They argue that "China as we know it is likely to unravel. Central authority will be diminished and regional hegemony will rise" (p. 83). In their view, political fragmentation and even violence are probable, with China becoming "a decentralized, highly federalized union" (p. 86).

These gloomy forecasts deserve serious attention, as they are based on a powerful methodology with a proven record of making accurate predictions in a variety of prior cases. The authors' basic approach builds on the theoretical insights of expected utility and median voter formulations, and it employs the deductive logic and analytic rigor of formal modeling. This approach is used to process the opinions of area specialists, seeking to draw out and explicate the implications of their views. These implications address the mutual partisan adjustment among various Chinese factions with different resources and preferences, as well as the dynamics of their interaction producing some eventual policy equilibria.

This methodology led Bueno de Mesquita and colleagues to predict that China's policies will erode the institutions of personal freedom and rule of law in Hong Kong in a matter of several years after 1997. Their analysis, however, does not indicate the distribution of alternative scenarios with their associated probabilities. One does not know, therefore, how much more probable is the authors' most likely scenario compared to other possibilities. For instance, what are the odds of Hong Kong becoming more like Shenzhen and Canton (p. 11) relative to the odds of it becoming more like Singapore (a city-state characterized by a large public sector, state capitalism, soft authoritarianism, and a thriving economy with a large presence by multinational firms)?

The authors report that despite the differences in expert opinions used as raw data in their analysis, the logic of their model has consistently yielded the same conclusions (p. 82). This consistency lends confidence to their results, which are less likely to be due to the idiosyncrasies of particular specialists whose views were consulted. It would, however, be useful to undertake sensitivity analyses, seeking specifically to test the resilience of the authors' predictions given alternative parameters or developments. Which factors will be decisive in changing their predictions? What will have to occur in order for them to alter their predictions? It would seem that a respect for private property and the rule of law (p. 46) are in themselves neither necessary nor sufficient for superior economic growth.

By the latter measure, Britain compares unfavorably with both Hong Kong and China. If British rule and institutions have been important to Hong Kong's past economic success, why has Britain itself fared so poorly in the recent past? Conversely, Chinese tradition does not necessarily augur continued authoritarianism. To the authors, "the essence of Chinese political history is that governments and ruling political parties are unlimited and that individuals have no rights" (p. 46). Yet, though being part of that history, the Kuomintang has taken significant steps in democratizing the political process on Taiwan. The influence of Britain's and China's political and cultural legacies form a crucial qualitative part of the authors' overall assessment of Hong Kong's future; this is not, however, a topic that they have chosen for systematic analysis.

The Chinese leaders do not operate in a domestic vacuum. Beijing's policies toward Hong Kong will at least in part reflect what significant foreign actors do or fail to do. Bueno de Mesquita and colleagues predict that international pressure on Beijing will be able to do little to promote free elections (p. 111) and will actually harm press freedom (p. 103) in Hong Kong. They suggest, however, international pressure will make a positive difference in the territory's judicial autonomy (p. 108) and academic freedom (p. 110). By differentiating issue areas and addressing "what if" questions, this part of the book is especially policy relevant. It would be useful to extend this inquiry to include constructive engagement and positive interactions, thereby providing a basis for assessing the effectiveness of these approaches relative to that of protest and sanction. It would also be interesting to know how or whether China is different from other countries in its susceptibility to foreign lobbying efforts in matters perceived to infringe on its sovereignty.

Sovereignty, national honor, and domestic legitimacy loom large in Enbao Wang's account. Hong Kong's return to Chinese jurisdiction is a highly important symbolic event, signifying a final end to an era of national humiliation due to foreign aggression and exploitation. It also is part of Beijing's long-term effort to reunify China—the "one country, two systems" policy was initially designed for Taiwan (p. 3). As Wang puts it, "the tradition of nationalism was the real crux of Chinese efforts leading to the 1984 Hong Kong settlement" (p. 24). Deng Xiaoping was quoted to say: "On the question of sovereignty, China has no room for maneuver. To be frank, the question is not open to discussion.... If China failed to take Hong Kong back in 1997, when the People's Republic will have been established for 48 years, no Chinese leaders or government would be able to justify themselves for that failure before the Chinese people" (p. 23).

Wang is much more sanguine than are Bueno de Mesquita and colleagues about Hong Kong's political autonomy and economic viability under Chinese sovereignty. His reasoning appears to be twofold. Economically, "it is unlikely that there could be a substitute for Hong Kong in terms of its role in China's modernization drive" (p. 153). Wang believes that the main thrust of this drive is unlikely to be changed and that Beijing will be self-deterred from changing its "one country, two systems" policy as "a decline of Hong Kong . . . would be costly to the mainland itself." Politically, he implies that Beijing will also find the continuation of this policy to be in its interest because Hong Kong's experience will be watched closely by Taipei and, accordingly, will have significant consequences for China's goal of reunifying Taiwan with the mainland (e.g., pp. 44, 50).

This rational, unitary actor approach to understanding Chinese policy overlooks the pervasiveness of partisan politics and factional competition analyzed in *Red Flag over Hong Kong*. Also, political or economic pragmatism has not always prevailed over ideological correctness or national fervor in Beijing's policy formulations in the past. The depth and breadth of support for China's current policies of economic modernization and "one country, two systems" are unclear. These considerations as well as the likely distributions of interest and power among various factions in post–Deng China could benefit from more attention, because Wang's optimistic forecast will ultimately have to be supported by such evidence.

As just implied, Wang (p. 204) agrees with Bueno de Mesquita and colleagues (p. 7) that Hong Kong's future will be determined less by its own internal developments than by events in China. Cheek-Milby's book, however, examines the role of Hong Kong's legislature from the early days of British colonial administration. In this system, the governor has overwhelming power, being accountable to neither a legislature nor an electorate. Indeed, members of the legislature have traditionally been appointed by him, and it was not until 1991 that some of its members were elected directly by the voters. As Cheek-Milby remarks, "up until 1984, the Legislative Council did not make policies or laws, it merely endorsed and ratified decisions that have been taken elsewhere" (p. 161).

Not until 1984—when Britain agreed to Hong Kong's reversion to China—were more popular participation and representation allowed. Beijing objected to these belated changes, which were made by Britain unilaterally and intended as a fait accompli to shape Hong Kong's political system in anticipation of 1997. The Chinese charged that Britain "lacked sincerity" and "had no right to transplant its parliamentary structure into what would be sovereign Chinese territory in three and a half years" (p. 244). Sino-British differences on this issue have become contentious. Likewise, Hong Kong's new legislature "is an increasingly fractious, divisive body, lacking in shared values" (p. 228). Some legislators (often appointed or indirectly elected members associated with business interests) caution against alienating China, while others (typically elected directly by voters) urge even faster and more thorough democratization before 1997. In the absence of a consensus, Cheek-Milby predicts that "the legislature will continue to operate as a marginal institution in Hong Kong's political system" (p. 228). A consensus on basic values, roles, and norms, however, requires time to develop and be institutionalized. As cooptation rather than representation has been the primary reason for Hong Kong's legislature during all but the last few years of Britain's colonial rule, the fragility and superficiality of recent reforms are not too surprising.

After Bipolarity: The Vanishing Threat, Theories of Cooperation, and the Future of the Atlantic Alliance. By Fred Chernoff. Ann Arbor: University of Michigan Press, 1996. 303p. $44.50.

Coping with Conflict after the Cold War. Edited by Edward A. Kolodziej and Roger E. Kanet. Baltimore, MD: Johns Hopkins University Press, 1996. 407p. $49.95.

William C. Wohlforth, *Yale University*

These two books tackle two contradictory trends in post–cold war world politics. On the one hand, the Western coalition of states not only has not fallen out over the spoils of its easy victory in 1989–91 but also plans to deepen integration in Europe and expand its military alliance eastward. Evidence for the emergence of armed conflict or militarized rivalries among the great powers is as scarce as it has been in the history of the modern states system. On the other hand, regional conflicts continue to erupt in armed violence within and among states, leaving behind thousands of casualties, millions of refugees, and legacies of poverty and hatred. Will regional conflicts spill over into relations among the great powers, or will the great powers' separate peace eventually infect the rest of the world? Chernoff assesses the prospects for NATO's survival after the demise of the Soviet Union and bipolarity. Kolodziej and Kanet and their dozen collaborators investigate the potential of the great powers as well as other actors to ameliorate ongoing or incipient regional conflicts. Together, the two volumes deliver a verdict (with important exceptions in the Kolodziej and Kanet collection) that mirrors the conventional wisdom among scholars of international relations today: The two trends of peace among the great powers and intermittent war elsewhere will continue to coexist as far into the future as anyone likes to predict.

Most scholars of international politics see NATO's survival and impending expansion as anomalies for neorealist power or threat-based models of alliances. Chernoff takes the analysis much farther and deeper. He compares the ability of models drawn from neorealism, neoliberal institutionalism, and cybernetic theory to explain variation in NATO cooperation from Suez in 1956 to Sarajevo in 1994. The analysis covers twenty-one cases in three broad issue areas over four decades and uses an effective combination of qualitative and quantitative techniques. None of the three models performs well. In the case of neorealism, for example, Chernoff shows that the anomaly is not merely NATO's survival after 1991 but, rather, the relatively low levels of cooperation in the alliance's early years (when U.S. hegemony and Soviet power and threat were all strong) and high levels in the latter years (reduced U.S. hegemony and Soviet threat). Chernoff constructs an alternative model of NATO cooperation that incorporates an eclectic mix of explanatory variables from the other models and naturally outperforms them decisively. The model generates his main conclusion: Alliance cooperation does not require a threat. Given high values on the other variables in the model—continued U.S. leadership, depth of communication among allied governments, and experience of leaders—security cooperation among NATO members can continue.

Chernoff recognizes the obvious criticisms: that a model tailor-made for a specific data set is bound to do better at explaining that data set than models relying on fewer explanatory variables drawn from very general theories; that his model says nothing about the origins of the alliance and can only project the possibility and not the likelihood of its survival; and that for these reasons his findings are likely to be of limited applicability to other cases. His answer is that he is less interested in evaluating general theories or explaining all alliances than in explaining cooperation in one of history's most consequential alliances, whose fate is a matter of great contemporary policy relevance. Given this refreshingly problem-oriented approach, however, it is fair to ask why the universe of explanatory variables was limited to those derived from the three general theories. Perhaps a model including factors they ignore, particularly variables indicating the changing domestic politics of NATO states, might do even better than Chernoff's "disjunctive, aggregative model of cooperation" and generate a less ambiguous forecast for the alliance's future. Bearing that uncertainty in mind, Chernoff's book nevertheless remains the best researched, most careful, and most systematic study of past and potential future security cooperation among NATO states. His basic conclusion, with its finely honed focus on a few variables of great importance for determining cooperation in the absence of a threat, should be given careful attention by scholars and policymakers alike.

The contributors to the Kolodziej and Kanet book would not be surprised by Chernoff's conclusion. Although some authors see signs of trouble on the horizon (particularly in Russia), they expect that continued cooperation among existing NATO members will take place in an overall context of peaceful relations among all the great powers. Their focus is on the inclination and capability of the major powers, as well as other key international actors, to provide a global order that will help resolve existing regional conflicts and limit the emergence of new ones. Overall, the forecast that emerges is gloomy. Jack Snyder's chapter makes the case most succinctly, but it is supported by other contributions, notably Patrick Morgan's essay on the United States. The very factors that lead to relatively low levels of conflict among the great powers—the spread of democracy and the waning power of imperialist social groups—make them incapable of supplying a global order that will substantially reduce violent conflict within and among other states. Democratic powers will probably not fight one another, but neither are they likely to make great sacrifices for world order. According to Snyder, "imperial political coalitions, which might invent interest in intervention, are not on the horizon for any of the great powers.... Likewise, strategic beliefs that would justify widespread interventions—the domino theory, fears of a radical bandwagon, the notion that peace is indivisible, obsession with reputational precedents—are currently weak or absent among the great powers" (p. 304). Democracy does not produce international altruism; modernization undercuts the social support for creating global or regional imperial orders.

Chapters on the United States, Russia, Japan, China, and the European Union either reinforce or do not contradict Snyder's basic contention. The United States is highly sensitive to the costs of world leadership, and the other powers define their interests in limited regional terms. Other contributions accept the conventional wisdom that the effectiveness of the United Nations and other international organizations in providing security and prosperity is strictly limited by the willingness of the major powers to cooperate, open up their wallets, and sacrifice lives. Essays on strategies of conflict management, from arms control to bargaining techniques, and Janice Gross Stein's chapter on overcoming enemy images highlight the difficulties of resolving conflicts once they have arisen. As far as the causes of regional conflicts are concerned, Mohammed Ayoob's chapter shows how these lie in deeply rooted domestic and regional dynamics, "which are largely autonomous of international control" (p. 242).

The upshot of these analyses is that we should focus our political and analytical efforts on how the current, relatively amicable relations among the major powers can be preserved rather than on pursuing the unrealistic goal of a global peace. This conclusion undercuts the basic premise of the book, and several contributors would disagree with it. Indeed, in the concluding chapter, Kolodziej refuses to accept the verdict of his own contributors and seeks to make the case for a slowly but inexorably expanding security community that will eventually engulf all the world's regions. This provides a useful intellectual tension, even if most readers are likely to be persuaded by the contents rather than the wrapping of this package. Overall, this is an edited volume of unusual thematic coherence and analytical breadth. To be sure, depth is sacrificed, but it is hard to see how it could have been otherwise. For a comprehensive stocktaking of the post–cold war international order and the problem of regional conflicts that presents the findings of many noted experts, readers could hardly do better than to consult this volume.

Together, the two books are not only important contributions but also revealing primary sources on what U.S. scholars of international relations are thinking these days. They reinforce the expectation that "present trends will continue." That expectation is usually right in the short term but always wrong in the long run. Whether Kolodziej's optimistic vision or the gloomier view of renewed great power conflict suggested by some of his contributors is the more substantiated view of the long run is a question the field still has to answer.

Policy Responses to the Globalization of American Banking. By Peter Dombrowski. Pittsburgh: Pittsburgh University Press, 1996. 247p. $34.95.

Benjamin J. Cohen, *University of California, Santa Barbara*

The foreign-policy implications of global banking have long fascinated students of international political economy. Almost from the moment banks first came onto the world scene, in thirteenth-century Italy, bankers have repeatedly crossed paths (and sometimes swords) with diplomats, as creditors or intermediaries. Even after the Peace of Westphalia formally enshrined the sovereign state as the key actor in world politics, private finance continued to play a prominent international role, sometimes operating in concert with public policy, at other times not. To the present day a natural tension exists between the interests of bankers and governments, ever intertwined but only partially overlapping. Peter Dombrowski's readable new book is but the latest addition to a vast literature on the dramatic interaction of high finance and high politics.

Dombrowski's focus is the United States. The book asks two questions (p. 5): "How have the international activities of American banks affected U.S. national interests? How have American public policy makers responded to internationalization?" Following an introductory chapter reviewing the "globalization" of U.S. banking since World War II and a second chapter setting the analytical framework, Dombrowski turns to five "critical episodes in the government's struggle with the consequences of internationalization for U.S. foreign policy" (p. 5): the Voluntary Foreign Credit Restraint program (VFCR) of 1965; authorization of International Banking Facilities (IBFs) in 1981; the International Lending Supervision Act (ILSA) of 1983; the Baker initiative of 1985; and the Brady plan of 1989. Each episode is examined in considerable detail. Lessons and implications are then summarized in a concluding chapter.

The book makes two significant contributions. First, a wealth of information is provided by the five case studies. Although no original archival research is offered, nor any econometric analysis of available data, it is clear that Dombrowski is thoroughly conversant with the existing secondary literature, has carefully studied relevant congressional hearings and other public documents, and tells a good story. Students and even specialists will find this narrative a handy one-stop guide to the nexus of U.S. banking and foreign policy over a crucial quarter-century of recent history.

Second, the author's analytical framework reformulates the book's central questions in terms of increasingly fashionable agency theory. The government's interactions with banks, Dombrowski argues, "are best understood as a *principal-agent relationship*" (p. 25, emphasis in the original). Government is the principal, with a mandate "to protect the public interest in the face of financial globalization" (p. 179). Banks are the agents, whose activities officials seek to "harness" (p. 179) to serve diverse foreign and domestic goals. Using this lens, Dombrowski confirms how the private activities of agents (banks) have frequently undermined implementation of public policy priorities; and how in turn the principal (government) has responded by attempting to alter the structure of incentives shaping international bank behavior. The approach is novel, even ingenious, and produces some interesting insights concerning information asymmetries, incentive design, and the problems caused by a multiplicity of either agents or principals.

In neither respect, however, is the book an unqualified success. One wonders, for example, what criteria the author had in mind in choosing the five cases that form the centerpiece of his discussion. What made these episodes most worthy of special attention? Were they somehow more "critical" than any other during this period? Were they representative of various "ideal types"? Or were they simply the most accessible in research terms? Dombrowski never tells us. Consequently, we have no idea whether the conclusions of his analysis may be marred by a selection bias of some sort.

One wonders also about the criteria implicit in the analysis of the five cases. References to "success" or "failure" abound—but success or failure as compared with what? Again, Dombrowski never tells us. The VFCR, for instance, which was meant to help stem the outflow of dollars from the United States, is said to have "failed to salvage the overall balance of payments program" (p. 64). Certainly, it is true that the external deficit continued to widen after 1965, leading eventually to even more draconian mandatory controls in 1968. But was this because of inadequacies of the VFCR, or was it due to other broader policy considerations (such as escalating expenditures for a war in Vietnam)? To answer the question, one would need to compare actual outcomes with an appropriate counterfactual: the author's best guess of what would have happened in the absence of the VFCR. Dombrowski offers no such comparison.

Likewise, Dombrowski speaks of "the failure of initiatives to manage the public debt crisis" (p. 120), which included ILSA as well as the Baker and Brady plans. But did they fail? It is true that many developing countries remained mired in debt even into the 1990s, with growth stymied and prospects poor, but as the author himself acknowledges, U.S. policies did manage to avert massive defaults or widespread bank failures. Contrary to the predictions of many, there was no meltdown of the international financial system. Surely, that might be counted as an achievement rather than a failure.

In part, of course, the answer depends on how one views the banking–foreign policy nexus in formal analytical terms. Dombrowski's use of agency theory simplistically assumes that government is necessarily the principal and banks the agents. Hence, success is implicit if public policy triumphs over private interests. But might the reverse be equally true, with government the agent of banks and success measured by the promotion of capitalist objectives? One need not be a Marxist to find evidence that could stand on its head Dombrowski's depiction of the agency relationship.

More to the point, one legitimately may wonder whether agency theory is appropriate at all in this particular context. Agency theory assumes both a hierarchy of power and some form of contracting between parties. Neither condition is apparent in the interaction between government and banks, where lines of authority are much less clearly drawn and where each side constantly attempts to manipulate the behavior of the other to suit its own ends. The relationship is clearly one of strategic interdependence between influential actors. Noncooperative game theory rather than agency theory might have better suited the author's objectives.

Hatreds: Racialized and Sexualized Conflicts in the 21st Century. By Zillah Eisenstein. New York and London: Routledge, 1996. 223p. $17.95.

Carolsue Holland, *Troy State University*

In this study of contemporary racial, nationalist, and ethnic conflict, the author attempts to go beyond traditional epistemological parameters by using feminist theory to explain why twentieth-century genocide, ethnic cleansing, and insurgency occur on a scale of brutality far more intense than at any time in the past. Her premises provide an alternative analytical basis to examine political violence in the twenty-first century.

Assuming that "there is no one kind of hate. No one kind of body. No one kind of psyche" (p. 23), the author explores hatred rather than its victims or those who attempt to mitigate it. In an effort to uncover the cycles and processes by which hatred is continuously reformulated, Eisenstein experiments with an approach to expand the traditional demarcations of social psychology. Consequently, purist psychologists and sociologists will be uneasy with her conclusions because they will not be comfortable with her assumptions.

Central to Eisenstein's thesis is that the tacit stereotyping of commonly held images—adversaries, allies, friends, enemies, leaders, followers—makes possible a general, widespread depiction of popularly held beliefs or public images. When Eisenstein takes the next step in the construction of a theoretic foundation to introduce what she calls "the body-mind dichotomy," her feminist suppositions deconstruct the traditional views of "psychological imaging." She says that men and women "image" differently. Perhaps this is so. But so far there are few if any empirical findings that validate this hypothesis.

Enter the feminist preoccupation with bodies—in this case human physical bodies, which "are always in part psychic constructions of meaning . . . [creating a] psychic realm [that] is a space of knowable and unknowable mental layerings that unconsciously and consciously frame our seeing" (p. 22). Eisenstein argues that these images of bodies are in large part used to assuage sociopsychological tension within a population organized as a circumscribed community. In so doing, she says, psychological imaging fashions what is inimical as well as what is self-confirming. What she means by "psychological imaging," however, differs from the traditional definition generally used by psychologists.

This is the crux of the difficulty with Eisenstein's thesis, as with many other feminist contributions: The deconstruction of terminology confuses the traditional theoretic relationships between process and structure. Accordingly, a reader who rejects feminist theory rarely stays with Eisenstein's arguments about rape and pillaging long enough to evaluate whether there is consistency between her conclusions and her interpretations. Eisenstein points out that experientially females conceive of and accept themselves differently from males. Women, she says, relate to the external world by projecting conceptualizations from their inner selves to the outside, whereas men tend to identify external realities that provide them with a starting point from which to begin to look inward.

In this vein, women readers of Eisenstein's book would generally accept the symbolic meanings imputed to the violent and brutal behavior of men, who over time are subjected to constant externalized reformulations of hate. She explains how Serbian nationalism brutalizes women by repeating the comments made by a lawyer and a judge, both Bosnian Muslim rape survivors: "By killing/raping older women, history is erased; by killing/raping young women, the future is wiped away" (p. 167).

But do these arguments engender reliable and valid findings if they are used as premises on which to build a case that feminism in the postcommunist world is a transnational "challenge to masculinist nationalism, the distortions of statist communism and 'free'-market globalism" (p. 166)? The conundrum can never be resolved because men cannot ever know what it is like to see reality from the persona of a woman. Neither do women—feminists or not—accept many externalized versions of reality which are uniquely masculine. Thus, there is no real way to assess either the validity or reliability of Eisenstein's findings.

At the end of the day, the quality of any scholarly work stands or falls on details in the text. It is unfortunate that many of the sweeping generalizations were not corrected by the copy editor. Specifically, such inaccuracies as celebrating the revolutions in eastern and central Europe in "the spring of '89" (p. 44) and declaring that "racism always differentiates in order to establish hierarchy" (p. 50) detract from Eisenstein's assertive, outspoken imaging of contemporary political violence as gender and racial differentiation that victimizes women at the same time it justifies masculine brutality.

Fairness in International Law and Institutions. By Thomas M. Franck. New York: Oxford University Press, 1995. 500p. $55.00.

Richard Falk, *Princeton University*

Thomas Franck has emerged in the 1990s as the most influential specialist in international law currently at work in the United States. Even before the attainment of such eminence, Franck had enjoyed a long, distinguished, productive career that included a lengthy stint as editor-in-chief of the *American Journal of International Law*. But it is his recent writing that has seemed to make the difference in his scholarly stature. First there was the publication of a widely discussed book, *The Power of Legitimacy among Nations* (1990), then his very influential and widely discussed article on democratic entitlement ("The Emerging Right of Democratic Governance," *American Journal of International Law*, January 1992, pp. 46–91), and now the book under review, which was honored by the 1995 prize of the American Society of International Law for the best published work in any language on international law.

What Franck has done brilliantly in this period is to write with authority and conceptual imagination about topics of great significance and interest but not effectively touched on by others, at least not from his direction, for a long time. No one before Franck has thought to write an entire book, and a long one at that, on the relevance of fairness to the ways in which international law operates. In this volume Franck gives us both a coherent and theoretically grounded perspective on fairness and a substantive look at its relevance for a wide range of international concerns, including peace and security, UN peacekeeping, environmental protection, international trade and development, the protection of foreign investment, and North/South relations.

The jurisprudential foundations for Franck's inquiry are mainly derived from H. L. A. Hart (*A Concept of Law*, 1961, pp. 208–31), and John Rawls (*A Theory of Justice*, 1971) but are adapted to the outlook of law and lawyers. Franck brings to bear a stress on principled decision making, consistency in addressing various analogous instances, and the reliance upon a discourse in law settings that is sensitive to the procedural and substantive requirements of fairness, both as perceived and as rendered.

To take but one among a myriad of examples, Franck shows persuasively that the International Court of Justice has been operating generally in accordance with fairness and deserves respect, having responded to a number of requests in settings which touched on sensitive and controversial issues, such as U.S. policy in assisting the Contras in their war against the Sandinista government and mining Nicaraguan harbors in the 1980s, or examining the authority of the UN Security Council to impose sanctions on Libya for its refusal to extradite individuals allegedly involved in the Pan Am jet explosion over Scotland. Using a theoretical framework to assess fairness, Franck proceeds to examine the extent to which concrete instances fulfill expectations about fairness.

Underneath this concern is a related preoccupation with the links between legitimacy and effectiveness and, more generally, between fairness and effectiveness. Franck is proceeding from the view that, in a legal order without strong enforcement mechanisms, legal effectiveness depends heavily on voluntary patterns of compliance by governments and other actors. Achieving compliance, as he shows, is more likely to occur if the law in its operations and its outcome is perceived to be legitimate and fair. In such circumstances, significant departures from applicable norms are likely to summon a strong hostile reaction from the organized international community as represented most characteristically by the United Nations.

Franck has written an important book that should be of interest and value to political scientists because of the issues addressed and the wealth of empirical material brought to bear. As indicated in a prefatory note, the book represents "a substantially revised and updated version" of Franck's 1993 course given at the Hague Academy of International Law. Such auspices imply a series of lectures on a fundamental theme of international law initially presented to a student audience of very mixed background. As a result, the book suffers somewhat from being longer than necessary to convey its main argument. It contains an array of impressive surveys of substantive developments in various subfields, but this material is presented in a manner that is often not integral to the main lines of Franck's argument on fairness.

There is a further, deeper problem. In addressing the development of fairness, Franck seems almost oblivious to the intrusion of geopolitics. He does not consider explicitly the problems specifically associated with legitimacy and fairness that arise from the extent to which dominant states throw their weight around. Despite a considerable emphasis on the United Nations, its procedures, and its undertakings, nowhere does Franck take on the extent to which the United States instrumentalizes that organization for its own foreign policy goals. Nowhere are examined the double standards widely believed to have discredited the UN Security Council's response to Israel's violations of its legal duties to the Palestinians in the Occupied Territories or the council's failure to notice Israel's covert acquisition of nuclear weapons while going ballistic when more ambiguous moves toward nuclearism are made by such countries as North Korea or Iran. Fairness is supposed to involve, in its most rudimentary form, as Franck acknowledges abstractly, treating equals equally!

There is one further source of concern. Why did not Franck relate his inquiry more directly to the seemingly parallel and much more fully elaborated scholarship associated with the New Haven School, headed by Myres McDougal and his principal collaborator over the years, the great political scientist Harold Lasswell. Much of what Franck depicts in relation to fairness is addressed by McDougal and collaborators in terms of correspondence with and expectations of reasonableness in a given context. Indeed, it would be illuminating to have Franck's views on the differences in jurisprudential orientation between the political language of reasonableness and that of fairness and legitimacy. One clear difference is that Franck's approach is much more rooted in positive law doctrine and practice, while the McDougal/Lasswell endeavor brings more centrally to bear the methods of social science and the relevance of social context, although both approaches seek to make the process and structures of international law more effective.

In conclusion, Franck has written a pathbreaking book on the issue of fairness, but its effect is likely to be constrained by these three clusters of difficulties. It leaves this reader in the end with a mouth too full of matters of marginal technical substance, and it fails, therefore, to provide a clear, sustained focus on the problematic of fairness in international law or a sufficient dialogue with other closely aligned scholarship.

Africa in the New International Order: Rethinking State Sovereignty and Regional Security. Edited by Edmond Keller and Donald Rothchild. Boulder, CO: Lynn Rienner Publishers, 1996. 253p. $49.95.

John W. Harbeson, *City University of New York*

Africa in the New International Order is a thoughtful collaborative effort to address the pervasive political and military realities of post–Cold War sub-Saharan Africa: (1) the limited capacity of African states to contain armed conflict within their respective borders and (2) the dimensions and dynamics of international involvement, both within and from outside the continent, in its management and mediation.

The volume is dedicated to Olusegun Obasanjo, "statesman, scholar, and patriot," who has applied his estimable abilities and energies to achieving political stability both in his native Nigeria and at regional and continental levels. Following Edmond Keller's lucid introduction of the volume's principal themes, Obasanjo offers a sobering balance sheet of benefits and liabilities accruing to Africa from the evolving post–Cold War international order. He argues that "more than halfway through the 1990s, Africa's situation has become more precarious and the prospects for the future bleaker by the minute" (p. 17). Notwithstanding a slowing arms race and democratic initiatives throughout the continent, Obasanjo and other contributors warn of increasing African marginalization in the forms of reliance upon "victorious" Western political and economic models thwarting the generation of those that might be home-grown, continuing adverse effects of the global economy on the continent's developmental fortunes, and the seeming ability of the Soviet bloc's residual legatees to outcompete Africa for scarce Western investment and development assistance.

In three respects, this volume does as much to open new vistas for international relations research in the African context as it does to explore them. The first lies in the interface between intra-African relations and the continent's external relations with the developed world. Against the specter of Africa's increased post–Cold War marginalization, seven case studies explore (1) the regionalization of domestic civil conflict in the Horn of Africa, provoked by the collapse of the Somali state and the birthing of the Eritrean state; (2) regional involvements to contain and mediate Liberia's civil wars of political succession, whose reverberations have been felt throughout much of West Africa; and (3) the implications for the political dynamics of the southern African region of South Africa's transformation from a malevolent pariah state to hoped-for, prospective benevolent hegemon.

These detailed and insightful case studies invite further and more systematic analysis directed to establishing a comparative politics of regional international relations based on their potential commonalities as well as their evident dissimilarities. As important as the potential benefits of the "horizontal" comparisons among regions are for both theory and practice, "vertical" comparisons of interaction patterns between evolving post–Cold war global economic and political regimes and dissimilar African regional theaters of international relations may be of equal or even greater importance. For example, short of full economic integration, how might African countries work to configure their regional international relations to minimize the collective marginalizing effects of global regimes and maximize their exploitation of whatever available opportunities these regimes may present?

The second area lies in the effect on, and potential opportunities for, African regional security initiatives of post–Cold War tensions in the policies of the major powers. Five chapters consider African regional approaches to regionalization of the continent's "insecurity dilemmas." They center on African approaches to these issues, notably the 1991 Heads of Government and state Summit in Dakar and the 1992 Kampala Conference on Security, Stability, and Development in Africa. Central to this evolving debate has been growing exploration of bases for limiting the principle of national sovereignty in the interests of collective security and, in Obasanjo's terms, "a common humanity." The twin realities are, on the one hand, in Rothchild's terms, "the weakening of the African state and the concomitant rise in ethnic, religious, and regional identity politics," and, on the other hand, the need to strengthen and improve upon the limited, imperfect security frameworks previously imposed by the principal Cold War adversaries.

The chapters on changing Russian and U.S. priorities are quite striking. Peter Schraeder's chapter on U.S. foreign policy suggests a contradiction between (1) increasing marginalization of Africa in political, financial, and bureaucratic terms, yet (2) growing de facto U.S. concern and involvement with African security dilemmas unleashed in part by the end of the Cold War. By contrast, Jeffrey Lefebvre's chapter on Russian post–Cold War foreign policy highlights diminishing strategic interest in African security dilemmas combined with residual, perhaps growing, business and economic interest in Africa. More than the other authors, Lefebvre opens up the whole issue of the connections between African post–Cold War security dilemmas and economic relations among African states and between African states and industrialized countries. The asymmetries among the policies of these and perhaps other major powers, the tensions within them, their effect on the continent's security concerns, and the opportunities these may present for creative bilateral and multilateral African foreign policies suggest new vistas for further research along the lines initiated in this volume.

The third area lies in exploring the relative merits and interconnections between normative and neorealist approaches to the post–Cold War study of African international relations. The prevailing theme in the analytical essays centers on the formulation of revised normative principles and guidelines as reference points for increasingly regional solutions to African security dilemmas, such as those outlined in the Kampala and Dakar conferences. But a second approach, suggested particularly by Zartman's chapter, may have more in common with Cold War neorealist theories of international relations. "Conflict," he argues, "is a constituent of African history [and] is so prevalent in African development because it is the means by which systems of internal and external relations are founded in the formative stages of new states' history. . . . [It] concerns real and important stakes. . . . Once institutions are developed and accepted conflict can be expected to decrease." Exploring the ramifications and implications of these alternative perspectives in an African context carries the potential for contributions to an analogous tension in more global approaches to the study of international relations in the post–Cold War era.

Autonomy or Power? The Franco-German Relationship and Europe's Strategic Choices, 1955–1995. By Stephen A. Kocs. Westport, CT: Praeger, 1995. 280p. $59.95.

David G. Haglund, *Queen's University*

As I write this, on a hilltop in Bad Godesberg, I can look out across the Rhine River to the Siebengebirge. In this part of the soggy Rhineland, when you cannot see those hills, that means it is raining; when you *can* see them, that means it is going to rain. They stand as a useful metaphor for contemplating the Franco-German relationship, about which Stephen Kocs writes so sensibly in this book, a relationship over which the clouds seem frequently to be lowering, with brief glimpses of sunlight suggesting the inevitable arrival of yet another political storm system. Illustratively, the most recent source of discord between the two central actors in Europe's integration drama concerns not a strategic question per se but an economic issue with vast strategic import.

Debate in Western Europe in the closing months of 1996 was dominated by the uncertain prospects of European Monetary Union (EMU). Were enough relevant countries going to qualify in 1997 for a common currency under the criteria established in the Maastricht Treaty, especially those relating to public finances (government deficits and debts, each measured against GNP)? If so, how could fiscal discipline be maintained and governmental backsliding into high-spending patterns of yore be avoided? If not, if currency union was to be stillborn, then what would this imply for the future of the European Union and for its "Carolingian" core, the Franco-German tandem?

To the German champions of EMU, above all to the government of Helmut Kohl, more has been at stake than a currency; after all, the Deutsche mark serves German interests admirably enough, and it is hard to claim that economic factors are necessitating its replacement. Instead, the project is eminently political, with high stakes indeed if you believe the chancellor, who has said on more than one occasion that EMU's—and by extension, European integration's—real import for the West Europeans is "nothing less than a question of war or peace in the 21st century." The EU summit in Dublin at the end of 1996 resulted in a compromise between the Germans, who wanted a "stability pact" adopted that could minimize the risk of their European partners' running deficits (and thus weakening the currency) after EMU has occurred, and the French, who just as understandably have been seeking greater room to maneuver through fiscal-policy tools.

Although it is defense cooperation upon which Kocs concentrates, the experience of the two countries in the monetary realm frames the central dilemma addressed in his book: Why cannot the Germans and French ever seem to achieve the degree of policy integration to which they profess to aspire? How has it come about, despite all the diplomatic and institutional gestures the two countries have made to each other during the past 40 years, that the Franco-German "axis" is today, the words of one former chancellor, "farther away from being a reality than it has been for decades" (Helmut Schmidt, "Nicht ohne Paris," *Die Zeit*, 20 December 1996, p. 1)?

An author steeped, as is Kocs, in international relations theory could take one of two broad approaches in answering these questions. S/he could wax theoretical, as is the fashion of so many (North) American political scientists who write about European security. Or s/he could, as is the custom of many Europeans addressing the same topic, tend toward the empirical, which in the circumstances can mean the historical. Kocs, an American, follows the European path and argues that the future of the Franco-German security relationship is only to be understood, if at all, by a careful reading of the recent past, particularly as concerns the security and defense aspects of the relationship.

As a political scientist Kocs does pay attention, early in the book, to the major theoretical combatants in the important dispute over Europe's future, but his treatment of them is brief and consists mainly in playing off the deterministic pessimism of structural realists against the optimism of writers inclined to see, both in the processes of historical learning and the phenomenon of institutional constraint, evidence that the future European order will be decidedly different from the balance-of-power rivalries of the pre–1945 era. His money is decidedly *not* on the structural-realist claim that a united Europe, or maybe just Germany, must loom as an inevitable challenger to U.S. power in the 21st century. Instead, he argues, "whether the process of European political union went forward or not, Western Europe appeared unlikely to emerge as an independent, militarily powerful strategic actor on the world stage" (p. 258). To advert to his title, it is certain that Europe will not constitute a pole of "power" in international politics, and it is unclear whether West Europeans will even achieve "autonomy."

It is as an historian that Kocs makes his real contribution to the debate on the Franco-German relationship. At the outset, he promises, in so many words, to supply the best account to date of that relationship, in the process providing a "higher degree of comprehensiveness" (p. 12) as well as a more sustained longitudinal analysis of the past four decades of Franco-German cooperation. The promise seems a little bold when first encountered, but Kocs actually delivers on it, through a meticulous reconstruction and impeccable analysis of the accomplishments and frustrations of the relationship over the past 40 years.

His attention is focused on the issues of conventional and nuclear defense during the cold war, as well as on the often neglected area of defense-industrial cooperation—all discussed with a view to the overall strategic ends the two countries have sought to further in their bilateral relations. Not surprisingly, since those ends have so often diverged, the promise of military cooperation serving as the locomotive of political integration has inevitably come up short, for as Kocs puts it so well, "from the late 1960s on, virtually all initiatives by Paris or Bonn for closer military ties were motivated by self-limiting objectives" (p. 254). The crux of the problem always lay in the inability of the two to agree on whether their own (and Europe's) security could be best ensured by strong transatlantic linkages within NATO (the German preference) or by a more autonomous "European pillar of defense" (as the French liked to construe it).

One wonders how Kocs's guarded caution on the prospects of bilateral defense cooperation will stand up, given the apparent recognition by France of the centrality of NATO to European defense integration, a recognition that would seem to remove much of the self-limiting dynamic of the relationship. More to the point, since one can scarcely take an author to task for not accounting for outcomes still in the future, it may be permitted to ask why Kocs should have told us (on p. 252) that, for Germany, "the Cold War was a period of gradual adaptation to the loss of great-power status." Most readers would have assumed it was World War II that wrought—and in a manner hardly gradual!—the altered status.

These and some other quibbles aside, this is an excellent book, to my knowledge the best treatment in English of what continues to be the core bilateral relationship in European politics today.

Beyond Sovereign Territory: The Space of Ecopolitics. By Thom Kuehls. Minneapolis: University of Minnesota Press, 1996. $42.95 cloth, $16.95 paper.

Ken Conca, *University of Maryland at College Park*

This short, provocative book embraces three ambitious goals: to problematize the twin discourses of sovereignty and governmentality that underpin mainstream thought about world politics; to identify the high stakes in the uncritical embrace of these discourses; and to assess the adequacy of current "ecopolitical" thought in moving beyond static, statist constructs. The book's title carries a double meaning: Global ecological interdependence makes it impossible to bound politics within the formal space of the state, and recognition of this fact forces us to reconceptualize a postsovereign world politics. For Kuehls, the state as governance mechanism cannot contain ecopolitics, and the state as concept cannot describe it. To problematize the state and point toward what lies beyond it, the author maneuvers skillfully through a wide range of political, social, and international relations theory: from Locke, Rousseau, and Nietzsche to Michel Foucault and Donna Haraway; from Kenneth Waltz and Hedley Bull to Richard Ashley; and from early naturalists such as George Perkins Marsh to contemporary "ecopolitical" theorists such as William Ophuls and Murray Bookchin.

At the heart of the book is a dual deconstruction of sovereign territoriality as an inadequate conceptual foundation for understanding world politics. For Kuehls, the mythic territorial state that populates most of international relations theory rests upon brittle discourses of sovereignty and governmentality, both of which are problematic in a world of tight ecological coupling. The author's principal contribution is to see the ecological problem of sovereignty as more than just one of artificial borders—that is, of transboundary pollutant flows and global commons that ignore political boundaries. The discourse of sovereignty is challenged by these realities, to be sure, but also by the inability of most states to harness nature to their purposes without external assistance or the coercive disciplining of local populations. Seen in ecological terms, the conventional image of the sovereign state is excessively sanguine not only about the fixed territoriality of the state but also about its governability. In juxtaposing the problem of sovereignty with one of governmentality, Kuehls adds a welcome dimension to the debate on ecology versus sovereignty, which too often sees as culpable the positional boundaries of states rather than the governance practices within them.

Kuehls also seeks to establish the stakes inherent in how we conceive of sovereign territoriality and whether we identify its practical and conceptual limitations. He contrasts two "eco-ethics"—a Lockean perspective on nature and property, in which the ecological, life-sustaining value of natural systems goes unacknowledged, and a "Nietzschean" perspective, which rejects both organic and mechanical conceptions of nature in favor of a more cautionary view: We had better be careful, because a chaotic, ever-changing natural world resists our management and has no particular need for our existence. If the Lockean ethic lies at the heart of our ecological dilemma, the Nietzschean offers a way out, provided that our conceptualization of world politics allows space for this normative stance. The critique of Locke's view of empty nature is well known, and there are shorter pathways to Kuehls's normative stance than his somewhat unconventional reading of Nietzsche. But the result is nevertheless a strong indictment of the managerial instincts that inform much of the "global governance" paradigm of environmental politics.

The book also includes an assessment of the adequacy of contemporary "ecopolitical" scholarship in capturing the essence of ecopolitics. This chapter is the book's least satisfying, perhaps because the burgeoning literature on ecology and political theory makes the task impossible. It also points to a larger problem in the text, however: There is a tendency to allow single thinkers to speak for, or at least bound, complex and multidimensional intellectual traditions. Thus, Locke alone speaks for the modern project, Waltz and Bull define the poles of mainstream international relations theory, and Ophuls and Bookchin bracket contemporary ecopolitical theorizing. Although the result is a telling critique in each case, one wishes that a somewhat broader range of thought had come under Kuehls's critical gaze: theories of interdependence or global civil society rather than the statist frame of reference shared by Waltz and Bull, for example, or the writings of social ecofeminists or chroniclers of green nationalism rather than the narrower statist/anarchist debate implicit in the work of Ophuls and Bookchin.

A second shortcoming is an unresolved tension between an evolutionary view of the environmental problematique and what is at times an ahistorical, absolutist critique of sovereign territoriality. Kuehls asserts that politics has already "abandon[ed] the space of the sovereign state" (p. xvii); we lack not examples but a way of thinking about them and an ethic to guide us through them. Yet, he acknowledges that the space beyond the state is populated not only by transnational activist groups such as Greenpeace but also by global capital. A brief discussion of the synergies and tensions between sovereignty and capitalism acknowledges the complexity of the relationship but sheds little light on how the state has adapted historically to transnational capitalism. The latter is not obviously less challenging to sovereign territoriality than contemporary environmental problems—leaving one to wonder whether the environmental dilemma has placed us in a fundamentally new world where our constructs no longer work, or whether we have been there all along. Other than the misguided premises of mainstream theory, what has sustained the state in the face of this challenge is not made clear.

One also wishes that the powerful and provocative examples—forest dramas in Amazonian Brazil and western Canada—had been developed more fully. The discussion of forest politics in British Columbia and the Brazilian Amazon illustrate Kuehls's themes, notably that the actors in these forest dramas operate in a space neither "domestic" nor "international" in the conventional sense, and that territory and people at the heart of each case have been constructed by governmental practices. Given the long history and deep roots of forest exploitation in both regions, however, one is left to wonder whether the state's demonstrated capacity to reproduce itself has been exhausted. The description of Brazil's forest politics also suffers from several inaccuracies that reflect the somewhat cursory discussion: Brazilian president Getúlio Vargas is referred to as "Getulia" (p. xiii); the state of Amazonas is confused with the nine-state legal jurisdiction known as legal Amazônia, which covers two-thirds of Brazil's national territory; and the number of forest dwellers in the region historically is dramatically understated.

These shortcomings notwithstanding, *Beyond Sovereign Territory* provides a valuable bridge between perspectives on the ecological problematique grounded in normative political theory and international relations. The author has done a better job of persuading that something important lies beyond sovereign territory than of conceptualizing what that may be, or clarifying how close to the edge we may have come. But the result is nevertheless a useful, challenging text that forces us to acknowledge the shifting character of sovereign territory in an ecologically interdependent world.

Parity and War: Evaluations and Extensions of the War Ledger. Edited by Jacek Kugler and Douglas Lemke. Ann Arbor: University of Michigan Press, 1996. 383p. $52.50 cloth, $24.95 paper.

Gerald Schneider, *University of Stuttgart*

One of the more dubious European contributions to the theory of international relations is the claim, vigorously advanced by Metternich, Bismarck, and others, that a power balance is a major precondition of peace. This central realist tenet is, at first sight at least, so convincing that it laid the foundation for some astonishing careers in public and academic life during the cold war. At the climax of this last major power contention, however, an American political scientist of distinctively European origin, A. F. K. Organski, questioned the often self-congratulatory wisdom of the political elites. He maintained that balance-of-power theorists do not pay sufficient attention to change and systematically underestimate the destabilizing effect of differential growth patterns among major powers. According to Organski's theory of alternative power transition (or power parity), war becomes more likely as the capabilities of the contending states converge. Twenty years after his first objection to balance-of-power theory, Organski and one of his students, Jacek Kugler, tested the alternative conjecture in a rigorous fashion. The main results of this collaborative effort, *The War Ledger* (1980), undermined the surprising renaissance that balance-of-power theory enjoyed during the neorealist 1980s. The study confirmed that power preponderance is a major pacifying force, while parity is associated with war.

The new collection by Kugler and Lemke pays tribute to what will arguably remain Organski's most important contribution to the study of international politics. *Parity and War* is one of those rare festschrifts that contain more than the marginal offshoots of the authors' current preoccupation. On the contrary, the editors have done an outstanding job in soliciting refreshingly critical evaluations of the extant scholarship and useful amendments to the original thesis. The volume starts with several appraisals (Lemke and Kugler, Vasquez, Siverson and Miller) and empirical tests of the parity hypothesis (Lemke, Kim, Houweling and Siccama, Danilovic, Geller, Wayman, Thompson, Werner and Kugler), moves on to assess the effect of nuclear weapons (Kugler, Zagare), and ends with several formal extensions of the original framework (Bueno de Mesquita, Kadera, Morrow). The conclusion takes the form of informed speculations by Organski and Tammen over the role the United States could and should play in the post–cold war era. Only one contribution, Arbetman's chapter on the economic consequences of the Civil War, dos not fit well into this impressively coherent book. As it has to be expected, most findings support Organski's vision, showing that the parity hypothesis also seems to receive empirical support for regional powers or that military buildups influence the timing of power transitions. Yet, some contributions, especially the ones by the formal modelers, also point out that the original thesis has to be qualified in different ways. Kadera, the author of one of the most interesting chapters, even suggests that the conflicting views of power transition and balance-of-power theory can be reconciled if a dynamic perspective is introduced.

It belongs to the character of such a research effort that many contributors are either former students or collaborators of Organski and Kugler; other authors belong to the elite in quantitative international politics and thus share the basic epistemological premises of the authors of *The War Ledger*. The obvious advantage of recruiting adherents of a sole research program is that the collaborators start from a unifying perspective. In the case under consideration, this achievement can, however, also be seen as the volume's main drawback. To put it metaphorically, a reader who wanders through the pages may sometimes feel like the proverbial explorer who cannot see the forest for all the trees. Although the editors' introductory chapter tries to integrate some of the findings, the reliance on different modeling techniques, samples, or periodicities leads occasionally to some repetition in the presentation of the basic theoretical argument and, more important, even yields slightly contradictory findings. Unfortunately, power parity theory, unlike rationalist deterrence theory, has not yet experienced the convergence toward a basic model on which most future work could be build. Furthermore, although some chapters offer a comparative theoretical evaluation, it is difficult to see beyond this alternative realist framework and to assess the relative importance of the power parity perspective.

The explanation pioneered by Organski is undoubtedly one of the few research traditions in quantitative international relations that is based on relatively solid empirical foundations, but it is not the only one, and it would have been interesting to note how this research program compares with alternative explanations. Interestingly, a recent evaluation shows that the power parity explanation scores high in comparison with two other research traditions. These other "families" are the democratic peace hypothesis and the more contested view that some form of capability concentration in the international system is linked to war. The balance-of-power thesis is one expression of the latter (Stuart A. Bremer, "Power Parity, Political Similarity, and Capability Concentration: Comparing Three Explanations of Major Power Conflict," manuscript, 1996, Binghampton University). Instead of engaging in a fruitful comparison with other theoretical perspectives, the balance-of-power perspective often serves as a theoretical strawman.

All in all, *Parity and War* is a useful addition to the growing number of studies that move beyond the noisy sterility of the interparadigmatic debates and explore realist conjectures and their possible alternatives in a rigorous fashion. The repetitiveness and partially contradictory results are probably only a sign that the field of quantitative international politics is gradually moving toward a more mature state. As this volume demonstrates, cumulativeness also means that the scientific discussion becomes less dramatic but simultaneously more nuanced.

Cooperation under Fire: Anglo-German Restraint during World War II. By Jeffrey W. Legro. Ithaca, NY: Cornell University Press, 1995. 255p. $35.00.

Lisa L. Martin, *Harvard University*

During war, states sometimes do the unexpected. In *Cooperation under Fire*, Jeffrey Legro studies one of the most unexpected of state acts under duress: restraint in the use of weapons that may offer them a military advantage. Legro persuasively argues that neither calculations of military advantage nor the international norms of warfare can explain variation in states' use of "unthinkable" weapons. Instead, he argues, we must turn to the culture of military organizations to explain why, during World War II, states sometimes accepted restraints on the use of particular weapons and at other times neglected all such constraints.

Legro pursues his argument by intensive and original analysis of British and German use, or nonuse, of unrestricted submarine warfare, strategic bombing, and chemical warfare. These accounts are complemented, where appropriate, by studies of U.S. and Soviet decisions. The research in this book is impressive, drawing on both secondary and archival materials. Legro has uncovered evidence about state motives and decision making that will have to be taken seriously by those who wish to understand either the history of warfare during the twentieth century or the problem of international cooperation during war.

The analytical problem is stated clearly, and the methods used allow a good test of alternative hypotheses. Legro develops predictions about the use of weapons based on three perspectives. The first, realism, asserts that states will use weapons if they calculate that such use will provide them with a relative military advantage. A major contribution of Legro's book is in showing that this perspective often fails to predict state decisions accurately. London was the most vulnerable city in Europe to strategic bombing, yet Britain turned early to this form of warfare. Germany had a technological edge in chemical weapons, yet it refrained from using them.

Legro also demonstrates that prior international agreements had only marginal implications for the pattern of weapons use. Under the strain of all-out war, such agreements influenced primarily the form, not the content, of state policy. While one might quibble about Legro's operationalization of institutionalism, the evidence here makes it difficult to argue that international institutions had much influence under these extreme conditions.

Legro proposes that organizational culture, defined in this instance as the beliefs and customs of national militaries, determined state preferences and thus state behavior. Strategic interaction with adversaries mattered, in that once a particular weapon was used, other states would respond in kind. But the initial decision to escalate had its origins in military beliefs. Thus Britain, with its long tradition of a battleship culture, denigrated the capabilities of submarines, although objectively it could have gained an advantage by turning earlier to unrestricted submarine warfare. State preferences, under conditions of limited information, were determined by military tradition. The evidence marshaled in this book elegantly traces the influence of tradition on preferences, finding only minor historical anomalies confronting this perspective.

The analytical framework Legro develops presents two primary questions, one about the relation between state decisions and military beliefs and the other about the sources and nature of organizational culture. Regarding the former, a student of institutions or foreign policy must ask about the conditions that allow a particular bureaucracy to have such a dominant influence on state decisions. Legro presents plausible arguments for why the military should have so much influence during wartime, such as the state's demand for military expertise and thus reliance on military assessments of the costs and benefits of alternative weapons. Yet, this argument begs the question of the determination of state preferences when military preferences are split. The battleship culture was dominant within the Royal Navy, but there was also a group of "submariners." Why did the battleship proponents have more influence on state decisions than the submariners? On this point, the framework here suffers from the same weakness as other models of bureaucratic politics. It tells us a great deal about what bureaucracies want but less about the question of which bureaucracy wins.

The second core question raised by the model of organizational culture regards the sources and nature of culture. On its sources, Legro does not attempt to provide a general framework for predicting culture. He rejects the position, long associated with Graham Allison, that "where you stand depends on where you sit." Instead, the process of generating an organizational culture is highly path dependent. Only a close look at the history of particular organizations can provide insight into the culture that develops.

The primary limitation of the organizational culture model, and the major challenge that Legro's work presents for those developing cultural theories, has to do with differentiating culture from preferences and developing a causal account of the relation between the two. Legro claims that preferences are determined by culture, positing a causal relationship. He criticizes dominant theories of international cooperation for treating preferences as exogenous, proposing a "two-step" model of international cooperation that requires a theory of preference formation. His proposed source of preferences is organizational culture.

Presenting a theory of preference formation requires that the analyst specify an explanatory variable clearly distinct from preferences and demonstrate a causal linkage between the two. Legro's explanatory variable is organizational culture. Yet, the difficulty of discerning the nature of causality arises when we look closely at the definition of organizational culture, namely, beliefs about how a group should respond to the outside world, in this case about the feasibility and legitimacy of using particular types of weapons. Defined in this way, the notion of organizational culture becomes uncomfortably close to a description of preferences, suggesting that the relation between the two is definitional rather than causal. Are preferences *explained by* culture, or *are* they culture? The too-close connection between the two is exemplified by the fact that Legro slips easily and often into discussing "organizational preferences" rather than organizational culture as his explanatory variable (see, e.g., pp. 18 and 27).

If organizational culture amounts to the same thing as organizational preferences, we do not have a theory of preferences. We may have a persuasive account of how military preferences shape state preferences, but the contribution of "culture" to this explanatory scheme requires further elaboration. Legro's careful, insightful, and intriguing study shows that military preferences have a powerful influence on state policy during war. *Whose* preferences matter for state policy, and the origins of those preferences, remain the challenges presented by this important book.

Bombing to Win: Air Power and Coercion in War. By Robert A. Pape. Ithaca, NY: Cornell University Press, 1996. 366p. $49.95 cloth, $19.95 paper.

Stephen A. Garrett, *Monterey Institute of International Studies*

The famous Italian air power theorist, Giulio Douhet, writing in the years after World War I, confidently predicted that at the outbreak of the next great war, a massive aerial bombing attack on an enemy civilian population would be enough in itself to induce surrender: "To put an end to horror and suffering, the people themselves, driven by the instinct of self-preservation, would rise up and demand an end to the war" (*Command of the Air*, 1942). The British head of Bomber Command in World War II, Sir Arthur Harris, echoed Douhet's theory when he argued (in December 1943) that a continuation and expansion of British city bombing over Germany would produce within four months a situation whereby, with half of Germany's principal towns destroyed, there would be "little doubt" of a German surrender. Harris's argument was that if Bomber Command were given the resources to flatten half the German cities, there would not even be a need for Operation Overlord, the planned invasion of Europe by Allied forces, except as a sort of mopping-up exercise.

The basic purpose of Robert Pape's intriguing study of strategic air power doctrine in the twentieth century is to consign Douhet, Harris, and their cohorts to the dustbin of history, or at least to the dustbin of discredited theorists. The author's brief, however, goes beyond simply rejecting the notion that by terrorizing civilian populations through air strikes a belligerent state can be forced to surrender. If Pape had confined himself to this point, then his book would be of relatively little interest, since there already exists a very wide literature (the United States Strategic Bombing Survey after World War II being a prime example) effectively demolishing the belief that by shattering civilian morale a government can be led to abandon its war effort. Pape instead questions the whole theory of strategic bombing itself, even when it is focused on relatively precise military or industrial targets. As he says (perhaps a little too boldly) in his conclusion, when all is said and done, "strategic bombing doesn't matter" (p. 316).

In arriving at this judgment, Pape first develops a general theoretical framework and then uses it to analyze five major cases of strategic bombing in the twentieth century: Allied air attacks on Germany and Japan during World War II and U.S. use of air power in Korea, Vietnam, and the Gulf War. His central theoretical device is the idea of military "coercion," which he defines as "efforts to change the behavior of a state by manipulating costs and benefits" (p. 4). In assessing air power as a tool for coercion, he further delineates four broad strategies that may be or have been adopted by warring parties: (1) punishment, the intentional targeting of civilians; (2) denial, attacks on the enemy's military and industrial base; (3) risk, a gradual escalation of bombing as a way of "signaling" the enemy in the mode of Thomas Schelling's *Arms and Influence* (1966); and (4) decapitation, assaults on the enemy's central command structure and even specific individuals.

As noted above, Pape's central conclusion is that in none of his case studies was strategic air power, strictly defined, a decisive factor in "coercing" the enemy to make peace. This does not mean that he simply dismisses the utility of air forces in bringing wars to a successful conclusion, but he does suggest that air power almost always has its most pronounced effect in theater operations, that is, in direct attacks on the enemy's combat capabilities. The basic premise behind this assertion is that the decision making of belligerents in wartime is above all influenced by the capacity of their armed forces to continue to offer a coherent military challenge to their adversaries on the battlefield. Economic dislocations at home as a consequence of strategic bombing may be adjusted for, civilian discontent may be dealt with or hardly arises, arms production can be creatively maintained even under a hail of bombs.

The case Pape offers against the enthusiasms of strategic air power theorists is, on balance, a strong and convincing one. This, indeed, is quite a good book by any standard and will have to be taken into account even by those who may dispute its basic thesis. It is very nicely written, well organized, and impressively researched, and it has a clear theme that binds together its disparate parts, especially the quite different case studies. Having said this, the book's defects, such as they are, may be said to be a reflection of its virtues. The constant drumming on the concept of coercion begins to pall over the course of some 300 pages. The author also tends toward excessive formalism in his use of lists; for example, there are always "three factors" explaining this or "four elements" underlying that. At times, certain points tend to be repetitively reintroduced, and in this respect the text could have been tightened a bit.

Perhaps the most substantive criticism is that, as for any essay which attempts to develop a central thesis, there is the danger of oversimplification. Particularly for those writing on military matters, the constant challenge is to appreciate fully the "fog of war" and the myriad diverse factors that may produce a particular decision. The conclusions Pape draws from his case studies are carefully considered, and there is no evidence of a rush to judgment. Still, the carping critic will always find some targets in a work of this kind. Pape's assertion that Hitler's domination over German decision making in spring 1945 was significantly less than it had been earlier is one such example. His argument that dropping the atomic bomb on Japan was not the decisive factor in Tokyo's choice to surrender is perhaps another.

This is an important book, however, and perhaps especially so for an American readership. In the aftermath of the Gulf War, the conventional wisdom in many quarters of U.S. opinion is that strategic air power was the "magic bullet" that vanquished Saddam Hussein and may serve a similar function in future crises. Pape is not exactly telling us to beat our B-2's into ploughshares, but he does suggest that war has never allowed of easy or cheap solutions and is hardly likely to do so in the future. This cautionary reminder is reason enough for the book having been written.

Crisis Bargaining and the State: The Domestic Politics of International Conflict. By Susan Peterson. Ann Arbor: University of Michigan Press, 1996. 208p. $42.50.

Paul Diesing, *State University of New York, Buffalo*

This well-researched book calls attention to a somewhat neglected factor in international crisis bargaining: domestic politics. Of course, studies of crisis bargaining usually have included domestic politics but not as the main focus of attention. Here we have an organized, systematic treatment of the domestic politics factor, rather than the casual, incidental treatment that often appears in case studies.

Peterson begins with the normal tactic of pushing existing theories—"conventional wisdom"—to the fringes to make room for her theory. She uses the standard tactic of oversimplifying each theory, sometimes to the point of caricature. Whether game theory, cognitive psychology, motivational psychology, or bureaucratic politics, all are reduced to one or two "variables," which are expected to predict the outcome of any crisis. Not surprisingly, the theories all fall short; they are "unable to make determinate predictions" and so must be replaced by Peterson's theory. For example, cognitive psychology consists of the teaching that decision makers tend to interpret new information in terms of their existing beliefs about international politics and about specific actors, more or less. But that cannot explain a specific bargaining process (pp. 17–18)!

Fortunately, Peterson's theory is broader and richer than these outdated theories. She distinguishes four types of domestic political structures, each producing a different kind of bargaining behavior. She also brings cognitive and bureaucratic theory in as enriching variables (p. 93); domestic politics provides the conditions in which these two variables are also sometimes relevant. Also, her theory does not have to make determinate predictions in all cases; other factors influence the process and outcome (p. 193).

A possible reason for Peterson's caricaturing of other theories is her philosophy of science. A scientist is supposed to set up a hypothesis in which an independent variable always produces a specific effect on a dependent variable via one or more intervening variables. Then, the hypothesis is tested against data and pronounced either false, true for those data, or improvable. Such a philosophy can apply to astronomy but not to international politics (or even geology) because the institutions, beliefs, personalities, power distribution, and so forth, are too complex to be reducible to a few variables and because they are continually developing. In addition, rationality almost disappears from cause-and-effect science, and from Peterson's book, though it is a central concept in game theory and cognitive and motivational psychology. But the book is useful despite this philosophy.

Having set up her theory and the four conventional theories, Peterson tests them against three detailed case studies. Guess which theory comes out ahead, though in need of some improvement via further research.

The case of Fashoda in 1898 is well researched and shows clearly the central importance of domestic politics in the crisis. Game theory predicts the British win—the game is Bully—but leaves open the specific tactical moves and precise outcome; these are explained by domestic politics, as the case study shows. Peterson uses numerous French and British documents, plus the many published studies, to provide a detailed day-by-day account with political background.

Unfortunately, the Berlin case, 1958–61, is not well researched. The sources are all from the U.S. side, plus Khrushchev's memoirs and Embree's collection of official documents. There are no German or even British sources. Consequently, the account is superficial. It focuses entirely on moves along the coercion-conciliation dimension and misses the rationality aspect that was crucial in 1961: the re-evaluation of one's own goals and priorities and reinterpretation of the opponent's goals and priorities, based on probes and interpretation of his messages. Kennedy's July 25 speech on the Three Essentials, plus Fulbright's July 30 statement, convinced Khrushchev, correctly, that the United States would accept the Berlin Wall. Diplomatic reports to the United States by Fanfani, Nehru, and Segni helped clarify Kennedy's understanding of Khrushchev's goals, and Kennedy's communications by letter and via Kekkonen clarified Kennedy's goal for Khrushchev. Earlier, Ulbricht's probes had improved his understanding of U.S. priorities.

There is also no mention of the fact that all the conciliation in 1958–60 was irrelevant, because the two sides had incompatible goals and talked past each other. Each side believed it was defending the status quo, but the two status quos were quite different. Peterson describes Eisenhower as conciliatory and Kennedy as firm, but in terms of goals the opposite is true: Eisenhower held firm, and Kennedy adjusted his goals.

Nor is there any mention of the fact that the Wall was Ulbricht's idea, devised to solve the problem of East German defections to the West. In the August 3–5 meeting he convinced the other Eastern leaders to accept the Wall by asserting that GDR industrial deliveries to their countries would collapse by year's end without the Wall. Khrushchev, in turn, accepted it because he believed the United States would not object, based on Kennedy's and Fulbright's speeches; but he suggested a wire fence first to make sure.

Peterson deserves credit, however, for ignoring the fantasies of the Cold War fanatics among her list of sources, such as Jean Smith.

Perhaps Peterson's narrow focus on the coercion-conciliation dimension in all three cases is due to her definition of the "independent variable," bargaining strategy, as a position on that dimension (p. 42). After all, the cognitive psychology teaching applies to political scientists as well as politicians. We all look for, notice, and focus on data relevant to our own theory, and neglect or discard irrelevant data, more or less; and Peterson does, too. So, perhaps more thorough research would not have helped.

Finally, the case of Crimea in 1852 is a very useful corrective to the tendency of some game theorists to exaggerate the rationality of crisis bargainers. Here is a situation that rational bargainers could have settled peacefully, but it slid into war through misunderstanding, misinterpretation, cabinet squabbles, and public opinion demanding war. Peterson brings out the continuous influence of domestic and coalition politics, plus cognitive factors, in the Crimean case.

In summary, this book convincingly shows that domestic political structure and process are always relevant to crisis bargaining, sometimes are important, and on rare occasions are centrally important.

Playing by the Rules: American Trade Power and Diplomacy in the Pacific. By Michael P. Ryan. Washington, DC: Georgetown University Press, 1995. 228p. $42.50.

I. M. Destler, *School of Public Affairs, University of Maryland*

Over the past two decades, East Asia has been the most active arena for bilateral U.S. trade diplomacy as well as the most productive. Responding to demands from frustrated U.S. exporters and members of Congress, negotiators from the Office of the United States Trade Representative (USTR) have pressed Japan, Korea, and other Pacific Rim nations to open their markets to a wide range of foreign goods and services, from beef and citrus and cigarettes through wood products and pharmaceuticals to semiconductors and insurance. More often than not, they have won significant reductions in market barriers. More often than not, they have employed the authority granted by Section 301 of the Trade Act of 1974, as amended. Section 301 became a symbol of trade determination on Capitol Hill, with its provisions periodically enhanced. It became equally a symbol of "aggressive American unilateralism" to those on the receiving end abroad. Curbing U.S. use of this weapon (not sanctioned by the General Agreement on Tariffs and Trade [GATT]) was one of U.S. trading partners' primary objectives in establishing the World Trade Organization and its strengthened dispute settlement procedures.

Playing by the Rules examines this U.S. trade policy experience over the 1976–1990 period. Michael Ryan builds his analysis around forty cases in which the USTR pursued market-opening negotiations with Japan, Korea, and Taiwan. Only three resulted in no action. In the thirty-seven instances in which the United States won some change in foreign trade practices, Ryan finds a clear pattern: thirty-three involved U.S. industries with "high commercial competitiveness," and thirty-one had "high GATT regime utility," meaning they were based on violations of existing GATT rules or built on U.S. aspirations for new rules. Ryan also carries out "process-tracing case studies" in nine of the forty negotiations: six that involve "rule-oriented trade diplomacy," in which the issue was foreign compliance with existing agreements, and three that he labels "power-oriented trade diplomacy," with the United States using its geopolitical strength to challenge market barriers for which no agreed rules existed. (Included in the latter is the famous U.S.-Japan semiconductor case). The rules-based cases proved more successful. The USTR won foreign compliance with agreed changes in trade practices in five of them (in one case no rule violation was found), compared with success in just one of three "power" cases.

The conclusions offer a powerful counter to the arguments of those (such as Anne Krueger in *American Trade Policy: A Tragedy in the Making*, 1995) who depict the United States as abandoning the multilateral trade system for unilateral advantage. The USTR generally chose cases in which U.S. producers were highly competitive and hence could exploit the sought-after market opening; and it chose cases in which agreed international rules were at issue. The aim, and the result, was to strengthen U.S. exports *and* the global trade regime, just what the enlightened, pragmatic internationalist would wish. Ryan rebuts claims by other scholars that USTR was pursuing a high-tech industrial policy (only eight cases fit into this category) or that the aim and result were "relative gains" for the United States at other exporters' expense. Typically, the market opening achieved was extended to producers from all nations, not just the United States, so the multilateral trading system was strengthened, not undermined.

Ryan buttresses his conclusions with an institutional analysis of the USTR and the Section 301 process, with particular attention to the private lawyers who press cases for producer-clients. These lawyers use tried-and-true legal argumentation; the USTR officials want to win the cases they take up, and this proves much easier when a foreign nation is caught reneging on an explicit international obligation. Hence, their situational motivations drive them toward "playing by the rules." Ryan argues in a nicely crafted concluding chapter that the United States should "return to its rule-oriented strategy," from which the early Clinton administration sought to depart (p. 184).

Unfortunately, the book has problems which offset its very real strengths. One is occasional sloppiness, such as identical titles for the first two tables and the statement that the 40 cases were "settled in the 1974–1989 period" (p. 14). (The first was *initiated* in 1976; the last were *settled* in 1990.) More troublesome is Ryan's insistence on labelling them "*301* Trade Cases," when no less than 13 (including 11 of the 24 Japanese cases) were not pursued under that section at all! Ryan is correct to include these additional 13: They were basically similar, and adding them brings his study closer to encompassing the true universe of such disputes, as he notes (p. 15). But the 301 label misleads the reader to no useful end—why not simply call them market-opening cases? And the author compounds the felony by applying the label to

U.S.–East Asian textile-quota diplomacy (in which the Americans are the protectionists) (p. 23). In fact, in his understandable desire to apply his analytical framework to the region's rising trade giant, Ryan includes in *Playing by the Rules* a chapter on the U.S.-China textile dispute of the 1980s. Yet, there was available to him a major Section 301 case against China, initiated by the Bush administration in 1991 and resolved in 1992! This would have been similar in structure to the other cases he describes and hence more relevant to his central analysis.

Finally, the book fails to mention the comprehensive Institute for International Economics study published in 1994, encompassing *all* Section 301 cases from 1975 through 1992: Thomas O. Bayard and Kimberly Ann Elliott, *Reciprocity and Retaliation in U.S. Trade Policy.* (Ryan does include a footnote reference to an earlier Bayard-Elliott article.) Ryan agrees with their analysis in some respects and differs in others; had he ventured a comparative look, he might have sharpened his argumentation and broadened reader interest. (For example, Bayard and Elliott developed more nuanced ratings of individual case outcomes.)

Deciding to Intervene: The Reagan Doctrine and American Foreign Policy. By James M. Scott. Durham, NC: Duke University Press, 1996. 333p. $49.95 cloth, $18.95 paper.

David N. Gibbs, *University of Arizona*

The presidency of Ronald Reagan is frequently viewed as a major period of transition in the history of U.S. foreign policy, from the relatively restrained, détente-oriented policy of the 1970s to a much more assertive approach, one that directly challenged the Soviet Union. The centerpiece of U.S. strategy during this time was a series of interventions in Third World countries, collectively promoted as the Reagan Doctrine, whereby the United States sought to destabilize or overthrow several leftist or pro-Soviet regimes. James M. Scott provides an extended discussion of these interventions, using a comparative case study method. The book describes the ideas behind the doctrine as well as cases of U.S. support for proxy forces in Afghanistan, Angola, Cambodia, and Nicaragua. A chapter on U.S. involvement in Mozambique is also included, and this is presented as a case in which the United States opted not to intervene.

Scott's study advances an analytical framework termed the "shifting constellations image," which emphasizes interaction among the president, his advisors, bureaucrats, members of Congress, and interest groups in the formulation of foreign policy. Scott asserts that this approach is more apposite than approaches emphasizing the exclusive power of the presidency in the foreign policy process. Scott reaches several conclusions. First, the Reagan Doctrine was formulated and implemented under a relatively inactive president, thus intensifying the role of nonpresidential actors. Second, there was no "universal," ideal-type application of the doctrine, and in practice the doctrine was adapted to specific circumstances. Third, implementing the doctrine entailed a high level of bureaucratic infighting. In addition, Scott offers a normative assessment of the Reagan Doctrine, which is mixed. Despite a few misgivings, he reaches positive conclusions regarding the operations in Afghanistan, Angola, Cambodia, and Mozambique, arguing that U.S. involvement in these cases entailed a reasonable degree of restraint and contributed to regional settlements. Scott is somewhat more critical of the Nicaragua operation, noting the lack of restraint and the excessive zealousness of certain policymakers.

Overall, the book provides a useful overview of the Reagan Doctrine, pulling together a range of literature on the topic. It makes extensive use of material from official memoirs and press accounts as well as some declassified documents, mostly gathered from the National Security Archive in Washington, D.C., and the *Declassified Documents* microfiche collection. The book is clearly written and should be accessible to policymakers and other nonacademics. The emphasis on bureaucratic politics and the general messiness of foreign policy formulation serves as an important corrective to realist approaches that overemphasize "national interests" as causal factors. Yet, Scott does not make a persuasive case that his account advances significantly beyond the existing literature with regard to the facts of the case studies or the interpretation of these facts. There is insufficient effort to elucidate the theoretical significance of the case studies. "The underlying theme is that foreign policy is a messy, complicated affair involving many different actors" (p. 247); this is not a novel insight.

Scott neglects to discuss the issue of official secrecy and the methodological problems it poses for studies of foreign intervention. The issue is especially acute for research on the Reagan presidency, since this era was marked by a considerable augmentation in secrecy. The range of classified information was substantially increased, while much of the information that was released seemed of dubious reality. In 1987, the American Library Association issued a pamphlet with the (very apt) title *Less Access to Less Information by and about the U.S. Government.* Our information base on U.S. foreign policy during this period is, accordingly, very incomplete, especially in the areas of intelligence and covert operations. None of these issues receives any significant attention in Scott's study. Many of his conclusions—for example, that the United States consistently opposed the Khmer Rouge, that it facilitated a diplomatic solution in Afghanistan, that it declined to intervene in the Mozambican civil war—seem overconfident, given the incomplete nature of the documentary records. In addition, Scott relies too heavily on public statements by present and former government officials. For the most part, these official statements are used uncritically, without weighing the interests of the sources or the plausibility of their statements.

The book avoids some complicated issues regarding U.S. intervention. On Angola, Scott provides an overview of the historical development of U.S. policy toward that country, and he cites the memoirs of former CIA official John Stockwell, who once headed the agency's Angola Task Force. Scott neglects to mention, however, the most important assertion in Stockwell's memoirs: that U.S. intervention during the 1970s was a major factor, possibly *the* major factor, in triggering the civil war in Angola, and that U.S. intervention preceded Soviet intervention. On Cambodia, Scott repeatedly asserts that the United States opposed the Khmer Rouge forces, and that opposition was a basic tenet of U.S. policy in Southeast Asia. An important qualification is added, however: "Accusations that the United States had looked the other way or even actively assisted in the strengthening of the Khmer Rouge by China and Thailand had surfaced . . . and the American decisions to support the seating of the Khmer Rouge at the U.N. instead of the [pro-Vietnamese government] after 1978 were frequently criticized" (p. 101). This alleged U.S. support for the Khmer Rouge receives only one sentence of discussion; the allegations are never evaluated or discussed at length. On Afghanistan, several researchers (including Selig Harrison of the Carnegie Endowment and Steven Galster of the National Security Archive) provide evidence that hardline elements within the intelligence community and the Pentagon tried to sabotage UN–sponsored negotiations over an extended period and may have delayed the Soviet withdrawal. These claims are not mentioned at all. Indirect U.S. support for narcotics trafficking in Afghanistan, Pakistan, and Central America (and the very interesting bureaucratic feuds which resulted between the Drug Enforcement Administration and the intelligence community) receives only cursory mention.

Overall, *Deciding to Intervene* presents a sanitized account of the Reagan Doctrine.

Challenging Boundaries—Global Flows, Territorial Identities. Edited by Michael J. Shapiro and Hayward Alker. Minneapolis: University of Minnesota Press, 1996. 416p. $21.95.

Roxanne Lynn Doty, *Arizona State University*

"The crossing of borders always announces itself according to the movement of a certain step [pas]—and of the step that crosses a line. An indivisible line. And one always assumes the institution of such an indivisibility" (Jacques Derrida, *Aporias,* 1993, p. 11). *Challenging Boundaries* invites readers to cross an array of presumed indivisible lines and embark on a nomadic journey that traverses the boundaries of physical, temporal, and academic space. Across a vast and ever-shifting terrain that spans modernity, premodernity, and postmodernity, we move from India to Iran to the Aboriginal Community in Australia to the nonspatial flows where identities are constructed, deconstructed, and reconstructed. In a volume that gestures relatively briefly but sufficiently (two chapters out of eighteen) to the unhearing and exclusionary demigods of mainstream international relations, we move ever closer to (though never finally arrive at) an antidisciplinary nondestination that gives much needed space to new voices and alternative perspectives. It is important, however, that these new voices and perspectives do not seek to offer fixed and closed alternatives in the form of new paradigms or methods that promise theoretical assurances and secure foundations by which scholars can progress ever closer to solving major puzzles and uncovering ultimate truths. Rather, like the political movements to which William Connolly refers in his chapter, "Tocqueville, Territory, and Violence," these voices and perspectives seem destined to be always poised on "that precarious threshold of enactment" (p. 156). This, however, is not such a bad thing. They may remain vulnerable to dismissal by those whose categories they problemetize, but arguably because of this they also remain less prone to cooptation, which is perhaps a fate worse than dismissal.

The essays in this volume celebrate ambiguity, instability, and contestation not in order to suggest that students of global life throw up their hands in despair and walk away in resignation from the complexity and seeming intractability of the issues facing humanity today. Rather, ambiguity, instability, and contestation are celebrated because it is in the denial of them, in attempts at closure and certainty, that the real danger lies. This danger takes the form of intolerance and power lurking behind academic veils of tolerance and scientific, objective neutrality. The danger also takes the form of economies of violence masked as discourses which purport to describe the world as it is and predict the forces that threaten to disrupt its order. This volume calls upon scholars, especially those in international relations and comparative politics, to consider the consequences of and accept responsibility for their "reality" constructing practices that produce ambiguous, stable, and consensual centers only at the expense of ignoring the increasingly blurred margins that encroach ever frequently and intensely on such mythic centers. Herein lies a distinctly ethical imperative to the volume, an imperative that calls for the abandonment of what Derrida refers to as a "problematic closure," whereby a domain, territory, or field of inquiry is appropriated by a discipline as its own object that occupies a unified space and can in principle be closed. The chapters in this volume expose and break open this problematic closure, illustrating the numerous and varied aspects of the margins that must be ignored in order for any center to be constituted as such. This ethical imperative, which is especially evident in George's chapter on postmodernism and international relations, can be read as an answer to the worn out and uninformed criticisms that postmodernism affords no grounds, no standards from which any kind of judgment can be made. On the contrary, George's discussion of some of the major attempts to move international relations in a more critical direction suggests that the long-standing consensus in international relations regarding the nature of the "real" world is a prime obstacle to addressing the very issues that are of the utmost ethical import. It is worth pointing out, as Shapiro does (p. xv), that the inadequacies of the disciplines of international relations and comparative politics do not arise solely or even predominantly from recent global changes, such as the end of the cold war. Rather, these disciplines with their respective problematic closure have *always* been limited. The boundaries being challenged in this volume have always been challenged "on the ground," so to speak, ever since their first precarious tracings. The fact that these challenges are more readily heard today owes a debt to publications such as *Challenging Boundaries.*

The boundary challenges that this volume presents are varied but intimately interconnected. The eighteen chapters address a variety of concerns from perspectives that draw upon a number of disciplines which ultimately defy disciplinary boundaries. I would argue that the volume is antidisciplinary rather than interdisciplinary in that the latter implies an acceptance of existing disciplinary boundaries but calls for a sharing and integration across them. The former implies that all such boundaries are largely artificial and contingent and work to exclude voices which cannot find a comfortable home within the bounds established by the academy. Derrida's discussion of three types of "border limits" that together make a single braid is suggestive of what this volume is all about. The first type of border limit is that which separates territories, countries, nations, states, languages, and cultures. The second type of border is that between domains of discourse, that is, the disciplines that have been represented in the university, domains of knowledge or disciplines of research. The third type of border is that which passes between a concept and its other, "according to the bar of an oppositional logic" (Derrida, *Aporias*, 1993, p. 18). The chapters in *Challenging Boundaries* cross all these borders in such a way that the result is a single, complex, multifaceted, and intricately woven braid which reveals the problem at the core of any border or boundary, that is, the fact that boundaries are threatened from their inception. The ever-present possibility of crossing is inherent in the very institution of boundaries, which makes borders or boundaries inherently undecidable, blurred, and always challenged. This being the case, constant efforts in the form of a wide array of practices are required continually to reinstitute boundaries as natural and border crossings as unacceptable violations. The chapters in this volume challenge boundaries by way of examining a number of interconnected boundary-producing practices. By presenting in one volume a wide array of "cases," which to those who honor established disciplinary boundaries may seem unrelated, we are able to get a sense of the enormity, complexity, and power of boundary-producing projects.

For example, David Campbell challenges potentially critical international relations works for failing to cross that edge-line between the illusory safety of presence and the feared disorder of absence even while these works themselves point to the overdetermined nature of such boundaries. The ultimate retreat of potentially critical works to the presumed safety of accepted epistemological grounds does not, however, belie what their initial impulses already have suggested: The borders to which they retreat are already too porous to withstand even the slightest critical scrutiny without resort to intellectual realpolitik. These theoretical border challenges cannot be separated from the conceptual and territorial crossings illustrated in some of the other chapters. For example, it is the flows of humanity (gendered and racialized) across territorial boundaries discussed by Soguk, Xenos, and Pettnam and the complexity and precariousness of identities discussed by Krishna, Turnbull, Ferguson, and Shapiro that testify to the inadequacy of the works critiqued in the chapters by George and by Campbell. The volume can be read as a call to engage the concerns of those who live their lives, struggle their struggles, and engage in resistances far removed from the editorial board rooms of mainstream international relations and comparative politics journals and from those academics who in describing and theorizing "reality" actively participate in its creation. The volume is at least implicitly a call for relevance.

A common thread running through many of the chapters is a problematization of the state, its relationship to and implication in the production of identities and the many and varied practices which construct and reconstruct it, though never in a final and completed way. As Pettnam suggests, the state is always in the process of becoming. These chapters illustrate what a very complex, widespread, and violent project this "becoming" is. They also illustrate that it is a never finally completed project, a project whose results—while extremely powerful and indeed often fearsome—are always somewhat contingent and significantly are always results that may *not* be successfully effected, hence the opening for resistance in various forms. Another common thread is the sensitivity to flux, flows, and shifts and the recognition that the world "as it really is" and the identities of the world's inhabitants are always in the process of becoming rather than in a state of being.

As noted earlier there is an important ethical dimension to this volume. It is evident in Connolly's challenge to the boundaries that preclude possibilities of a pluralism which is not constrained by statist politics in its various guises; George's plea for international relations theory and practices that would facilitate openness rather than closure; Shapiro's call for investigations into constructions of self and otherness and how these are related to the emergence of global antagonisms; Soguk's focus on statist reterritorializing practices that discipline alternative modes of being in the world; Ferguson's explication of how militarized identities are produced in varied and often unnoticed ways and how this results in the glorification and legitimation of warfare itself; and the other chapters that cannot be addressed adequately in this review.

The book is important and should be read especially by those members of the academic border patrol who monitor the increasingly vulnerable border crossings where critical theorists relentlessly stage their attacks. The volume reminds us that challenging boundaries in all the ways outlined in the chapters strikes at the very core of international relations as an academic discipline and as a practice. Indeed, it reminds us that the academic discipline of international relations *is* a practice and a very political practice at that. For this reason we should not hold our breath too long waiting for a response from those who especially need to read this volume. Nor should this cause an excessive amount of alarm. It is the increasing support for and success of publications such as *Challenging Boundaries* that demonstrates the extent to which boundaries really do no longer hold and never did without the use of violence in its various guises. Like the Berlin Wall and other edifices of the very recent past, traditional international relations paradigms are crumbling, and while some may cling fearfully to their taken-for-granted premises, their commanding position no longer holds. *Challenging Boundaries* casts a shining light on the emperors' nakedness, though some will still refuse to look.

Refugees in Inter-War Europe: The Emergence of a Regime. By Claudena Skran. Oxford: Oxford University Press, 1995. 324p. $45.00.

Gil Loescher, *University of Notre Dame*

One of the more pressing problems of international relations today is the seemingly endless growth of forced displacement worldwide. Refugees are everywhere—a by-product of intrastate conflicts, massive human rights abuses, and disintegrating states and communities. Overall, according to the United Nations, there are currently about 14.5 million refugees and an estimated 23 million internally displaced persons.

While many people are inclined to perceive refugees as a relatively recent phenomenon that primarily occurs within the developing countries, the Balkans, and the former Soviet Union, the problem is neither new nor unique to the Third World or disintegrating states. *Refugees in Inter-War Europe* reminds us that between the world wars Europe experienced refugee flows similar to those taking place today. As is true for most contemporary refugee movements, people left home for varied and complex reasons, including the severe economic conditions and starvation that accompanied the violence and disruption of war, the upheaval of political and social revolution that followed the breakup of multiethnic empires, the outburst of claims for national self-determination, and the creation of new states. The majority of these people were members of unwanted minority groups or the victims of warfare, communalism, and indiscriminate violence. Then, as now, refugee movements not only significantly affected the domestic politics and local economies of host countries but also were the source of severe bilateral tensions between states. Essentially, the refugee problems of the 1920s and 1930s were political, as they are today.

Claudena Skran's book details the history of organized international efforts for refugees during the interwar period. She argues that an international regime was created in 1921 when the League of Nations appointed the first High Commissioner for Refugees, Fridtjof Nansen. Initially, league states mandated that aid be limited to Russian refugees, that league funds be spent only on administration and not on direct relief, and that refugee assistance be considered temporary. Over the next two decades the scope and functions of that regime gradually expanded as attempts were made to regularize the status and control of stateless and denationalized people throughout Europe. The effort grew to encompass refugee settlement, employment opportunities, emigration, and linkage of refugee assistance with economic development. The interwar period also saw the development of norms about the protection of refugees and the establishment of refugees as a special category of migrant within municipal and international law.

Despite Nansen's innovative and successful initiatives on behalf of refugees, international cooperation on the issue collapsed after the rise of fascist regimes in Europe. During the 1930s, there was broad consensus in almost every industrialized nation, particularly during the Great Depression, that national interests were best served by imposing and maintaining rigid limits on immigration; that international cooperation on refugees had to be limited by tight fiscal constraints and the need to employ one's own citizens; and that no particular foreign policy benefits would be gained from putting political and moral pressure on refugee-generating countries or from accepting their unwanted minority groups or dissidents. These views were fully operative in the United States and in other countries traditionally open to immigration. This led to the eventual breakdown of the international refugee regime, with disastrous consequences for millions of people.

The interwar efforts did leave one lasting and important legacy. Two decades of organizational growth and interstate collaboration had firmly established the idea that refugees were victims of human rights abuses for whom the world had a special responsibility. Moreover, the first international cooperation on behalf of refugees and the establishment and evolution of international refugee agencies provided the foundations on which successor institutions, such as the Office of the United Nations High Commissioner for Refugees (UNHCR), would build.

Skran's fine book teaches us a lot about international cooperation on the refugee issue. States have traditionally sought to preserve their territorial sovereignty by guarding their borders. Yet, in the aftermath of the massive dislocations of World War I and again in World War II, governments recognized that, although in a legal and a political sense they retained absolute power over immigration and refugee matters, their effective power to restrict transnational population movements was limited. Thus, the international refugee regime was established to serve the interests of governments and to facilitate both burden sharing and coordination of policies. Nevertheless, states remain the dominant actors and establish the rules and procedures within which most initiatives for refugees have to take place. The most significant institutional weakness of the regime is its dependence on voluntary contributions from states to carry out its programs. To initiate relief operations, the refugee regime must solicit funds from the industrialized countries and must obtain approval or acquiescence from the governments of both developing and developed states to operate within their territory. Moreover, the right to grant or deny asylum or admission for refugees to another country remains, as it did three-quarters of a century ago, the prerogative of sovereign states.

With the end of the cold war, there is less ideological motivation and strategic concern for states to cooperate on refugees. Thus, a danger exists that the international refugee regime will become increasingly ineffective and eventually collapse, as was the case in the late 1930s. If international organizations concerned with refugee issues are to respond successfully to the challenges of the post–cold war era, then they must demonstrate they can find some useful purpose in addressing the root causes of population displacement, which now more than ever clearly relate to the stability and security of states.

On the Causes of War. By Hidemi Suganami. Oxford: Clarendon Press, 1996. 245p. $62.00.

John A. Vasquez, *Vanderbilt University*

Hidemi Suganami has written a complex philosophical treatment of the idea of causation as it relates to war that is more subtle than it initially seems. He provides a new way of examining the past to learn how wars could have been avoided and why they were brought about. Although Suganami gives full treatment to the standard ways in which social scientists look at the idea of cause (with chapters on the questions to be asked, prerequisites, correlates, causation, and origins), in the end he makes the case for explaining the causes of war in terms of who is responsible for bringing about the war. This has important legal implications; as Suganami reminds us, Hans Kelson pointed out that the Greek word for cause, *aitia*, means guilt (as in the cause responsible for the effect) (p. 209).

Like any good social scientist, Suganami does not believe there are as many causes of wars as there are wars. He seeks to find the family resemblances among wars in terms of their origin. He says the patterns associated with war origin can be uncovered by focusing on six types of acts governments take in a potential war situation: (1) resistance, (2) acts with belligerent intent, (3) acts of contributory negligence, (4) insensitive acts, (5) thoughtless acts, and (6) reckless acts (p. 173). The last four are particularly important for exposing the "folly" of humanity and once understood could probably do much to reduce the incidence of wars by eliminating those that are a function of poor or reckless decision making. This is a more policy-oriented approach than the standard scientific approach of Lewis Richardson, who hoped to promote peace by identifying the general causes of war and then removing or mitigating them. Suganami's approach may have more immediate payoff if it can avoid succumbing to a witch hunt for war guilt that would destroy all hope for objectivity.

Suganami begins with a detailed treatment of Kenneth Waltz's *Man, the State, and War*, and those interested in Waltz's early work will find chapters 1 and 2 of this book of special interest. As one may expect given his normative concerns, Suganami finds Waltz's systemic (third image) emphasis woefully incomplete because "international anarchy makes no war inevitable." Since Suganami is interested in explaining particular wars, he obviously is not going to be satisfied with an explanation that is confined to necessary conditions and never specifies sufficient conditions; nor should we.

How we reason about war, then, will determine what we learn and how useful that knowledge will be. Suganami therefore spends a great deal of time looking at the problems, pitfalls, and opportunities in analyzing the necessary conditions of war, the sufficient conditions, and how particular wars come about (the historical origins). In this part of the book, more scientifically oriented readers will find various nuggets worth mining, although they will generally find Suganami's dismissal of Hempel's covering law approach as unconvincing because it is done on such a narrow philosophical base. Nevertheless, Suganami's distinction between necessary conditions that make war a *logical* impossibility and those that make war a *scientific* impossibility is crucial to keep in mind in constructing explanations, especially if one is wont to confuse the former with the latter, as is sometimes done.

Some of Suganami's most incisive comments about the importance of causal reasoning appear in his discussion of the literature on the democratic peace. He makes it clear that even if the evidence is accepted that democracies do not fight each other, this still does not constitute a necessary condition for peace; dyads that are not democracies (like the United States and Russia) also have never fought each other. He also helps us identify the various processes (and reasons) whereby a democratic dyad may be at peace other than Kantian processes. Sometimes it may only be a coincidence; for example, Latvia and Chile are both democracies and do not fight each other. At other times, it may be due to "collateral effects"; New Zealand and Australia do not fight each other because they share a British heritage. In other cases, a "reverse-causality" process may occur in which war imposes democracy, and the subsequent peace nurtures democracy (e.g., the United States and Japan since 1945).

Perhaps what is most worthy of serious consideration for practicing researchers is Suganami's claim that to explain war fully one must explain how a particular war came about. Many readers will find the chapter on origins of intrinsic interest. Suganami argues that to show (in Hempelian fashion) that certain correlates were present prior to a particular war does not uncover the causal process by which these factors brought about the war. To do that, he maintains, one must construct a story of the war's origins. Even though most scientifically oriented political scientists will find Suganami's own tack of looking for responsibility insufficiently nomothetic, his reflections on the need to construct story plots that explain how a particular end was causally brought about will prove stimulating to those interested in identifying the paths to war and in writing theoretical case studies.

In doing such research, however, most North American and "positivist" political scientists will insist on looking at the origins of specific wars in terms of what has been established (usually statistically) about the general factors that make war highly probable. Such political scientists still feel that a scientific cause cannot be inferred from a single case or from case studies that select on the dependent variable.

In this sense, some readers may want to place more emphasis than does Suganami on locating what he calls war-conducive mechanisms. He suggests that a variety of such mechanisms exist, and they simply need to be activated "when certain requisite conditions are met" (p. 172) to produce war. What these conditions are, he never states, but one obvious possibility would be to see these in terms of statistically identified steps to war.

Those engaged in peace research, and especially those concerned with normative peace studies or with doing empirical research in a postpositivist vein, will find this book of interest.

Schools of Thought in International Relations: Interpreters, Issues, and Morality. By Kenneth W. Thompson. Baton Rouge: Louisiana State University Press, 1996. 200p. $40.00 cloth, $14.95 paper.

David Wilkinson, *University of California, Los Angeles*

Kenneth W. Thompson has provided scholars, professionals, and students with a useful review of certain approaches, mainly American, to thinking about international relations. Both the review and the approaches may be called "traditional" in the sense that in them novelty is hardly sought; specialization is not praised; moral neutrality is not regarded as a moral virtue; method is important, methodology is not. Among the traditional approaches, Thompson prefers, as he has consistently preferred over several decades and 22 books, that approach he styles "realism," which he locates by contrast to "idealism" (liberal and socialist) and to "cynicism."

Current graduate students in departments where the newer approaches are dominant will probably find of greatest interest Thompson's stern criticism (pp. 120–4) of Keohane and Nye, *Power and Interdependence* (1977), and his lengthy critical comparison (pp. 129–44) of Kenneth Waltz's *Man, the State and War* (1954) with the same author's *Theory of International Politics* (1979), rather to the disadvantage of the later work. The examination in chapter 1 of the mid-century figures and influences of three university-centered schools of thought—Chicago (with vignettes of Charles Merriam, Quincy Wright, and Hans Morgenthau), Harvard, and Yale-Princeton—also may awake their interest, but few outstanding works outside the Chicago school are cited, and none is substantively reviewed except Morgenthau's.

Thompson's treatment of Reinhold Niebuhr, and to a lesser extent of John H. Herz and of Morgenthau, is attractively enough written that it may stimulate some readers to search out their work. In the case of Herz, Thompson's readers will be led, and I think quite rightly, to *Political Realism and Political Idealism* (1951); in Morgenthau's case they will not likely overlook *Scientific Man versus Power Politics* (1946) as a prelude to *Politics among Nations* (1948 and later editions). But in Niebuhr's case they will have to find *The Children of Light and the Children of Darkness* (1944) themselves.

Anecdote and substance are presented in a reasonably balanced way in Thompson's treatment of the writers just mentioned. The anecdotal references to such writers as Nicholas Spykman, Martin Wight, and Quincy Wright, which may capture student attention for a moment, could well have served as vehicles for imparting considerably more of these authors' substance. As regards one work which does receive substantive notice, it is probably too optimistic, though certainly scrupulously correct, for Thompson to refer readers from his two-page summation of Arnold Toynbee's *A Study of History* (1934–61) to the full twelve-volume edition of that work directly rather than by way of the one-volume Caplan (1972) or two-volume Somervell (1947–1957) abridgements.

Schools of Thought in International Relations is aimed at practitioner-professionals as well as scholars. This may account for the amount of attention Thompson affords the practitioner-theorists George F. Kennan and Paul H. Nitze. But given Thompson's eventual judgment of their work (p. 70), such detail seems inordinate, at least as compared to the brief notice given, say, Thucydides, Machiavelli, Castlereagh, Churchill, Nixon, and Kissinger.

Despite such reservations, the book serves well in recollecting voices once powerful, now less heard, and in making the case for long-lineage approaches in a field increasingly scientistic. Oddly enough, it seems most likely that *Schools of Thought in International Relations* will be used as a text in courses that focus mainly on the newer approaches, but only those that pay some heed to the achievements and the admonitions of the durable traditions.

Cooperative Security: Reducing Third World Wars. Edited by I. William Zartman and Victor A. Kremenyuk. Syracuse, NY: Syracuse University Press, 1995. 376p. $39.95.

Steven R. David, *Johns Hopkins University*

Since the end of the cold war there has been a surge of interest in the causes and cures of Third World conflicts. The reasons for this attention are not obvious. As Ted Gurr has observed, the number of conflicts in the Third World has not increased markedly since the collapse of the Soviet Union, and the countries they afflict remain overwhelmingly poor and weak. Nevertheless, with armed conflict increasingly occurring within states rather than between them, with internal wars wracking states on the doorstep of Europe, and with students of security studies freed from the constraints of worrying about "force-to-space ratios" on the Western front, it is perhaps inevitable that attention would turn to what previously had been a sideshow of international relations.

The volume under review brings together scholars from the former Soviet Union and the United States (many from the Johns Hopkins Nitze School of Advanced International Studies) to explore how Russia and the United States can cooperate in reducing Third World conflicts. The book is divided into several parts examining the concept of cooperative security, case studies of Third World conflict in which the former Soviet Union and the United States played a role, and the future of U.S.–Russian cooperation in the Third World. This last is what makes the book distinctive but also is a serious drawback.

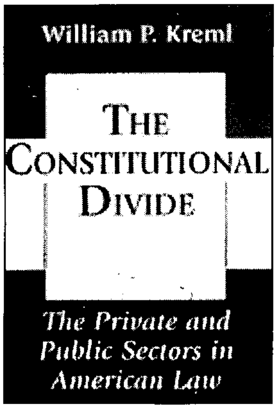
Perhaps when the book was first conceived, Russia (or the Soviet Union) was still seen as a major player in the Third World, and ways of linking it with the United States in a joint effort to deal with Third World conflicts made sense. At present, however, as many of the contributors to the volume acknowledge, Russia has neither the means nor the capability to influence events in the developing countries. Moreover, it is not likely to do so in the future. As Bruce Parrott argues (in one of the better chapters), internal chaos and concern with the "near abroad" will keep Russia occupied in the years to come. If in the future Russia moves in a Western direction it will defer to U.S. concerns in the Third World; if Russia turns hostile to the West it will lack the economic strength to project its influence far from its borders. *Cooperative Security* never makes a persuasive case for why we should care about Russian policy in the Third World.

The authors also fail to argue convincingly why the United States (or any great power) should seek to reduce Third World conflict. Zartman and Kremenyuk maintain that it is "screamingly urgent" for Russia and the United States to deal with wars in the developing world, but they present precious little information as to why that is so. There is some lip service paid to worrying about world stability and the spread of these conflicts, but too often it is simply assumed that because there are wars in the Third World it is in the interests of other states to stop them. For those who would like to believe that there are good reasons for the United States (and other great powers) to be concerned about instability in the Third World, there is little to support their views.

The case studies vary greatly in quality. The chapters by Doran, Thornton, and Prizel provide good background material on U.S.–Soviet relations during the cold war as well as interesting studies of regions often ignored by the literature (e.g., the Andean countries). Too many of the other chapters, however, have the feel of being hastily done, and little effort is made to integrate the chapters with one another. Few lessons from these chapters emerge, in part because of the tremendous variance of the U.S.–Soviet roles in the conflicts (high in the Middle East, low in Latin America) as well as the changed condition of Russia today. A great deal of attention is paid to U.S.–Soviet cooperation during the war with Iraq, but as many of the authors emphasize, that proved to be an unusual case with very little likelihood of its conditions being duplicated in the future. One is left wondering how the markedly different situation prevailing at the time of the cold war provides a basis for cooperation between Russia and the United States now that the cold war is over.

The section on implications fares a bit better, but here, too, there are problems. Mark Katz makes a valiant effort to employ Zartman's concept of a "hurting stalemate" (antagonists realize that continued conflict is not in their interests) to U.S.–Russian efforts at war resolution. But Katz's analysis ignores Christopher Mitchell's point in an earlier chapter that great asymmetries in power can be just as effective in promoting a settlement as a stalemate. Instead of promoting a deadlock, perhaps outside powers should act to create a greater *imbalance* of power. In what is perhaps the most intriguing chapter of the volume, Edward Kolodziej makes a plea for integrating Russia into a concert of great powers dedicated to preserving world order. Kolodziej draws an analogy with France in 1815 and 1945 in his appeal for the West to assist Russian development while incorporating it into Western institutions such as NATO or the OSCE. Despite the many unanswered questions raised by his argument (can outside states play a significant role in bringing about the political and economic development of Russia at an acceptable cost, can a concert of powers be maintained given the differing interests of the states that make it up), Kolodziej at least advances original and provocative views in a manner absent from much of the rest of this volume.

In sum, while there are flashes of insight and some good background material in *Cooperative Security*, the sum of its parts exceeds the value of the whole. Given the quality of many of the contributors, this is surprising. I suspect that if freed from the anachronistic constraint of providing Russia a central role in Third World conflict reduction, then the authors would have made a far more valuable contribution to the growing literature in this field.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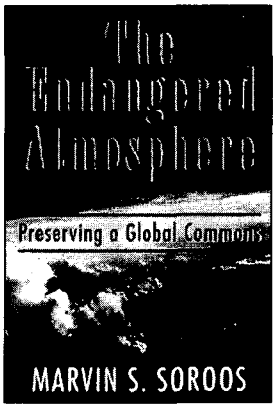
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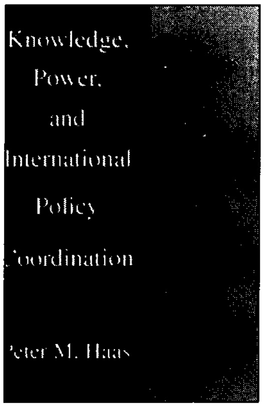
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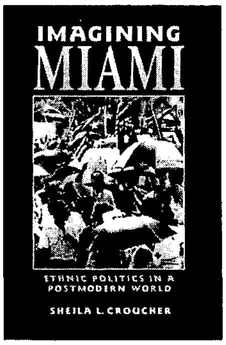
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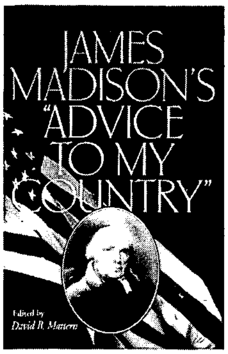
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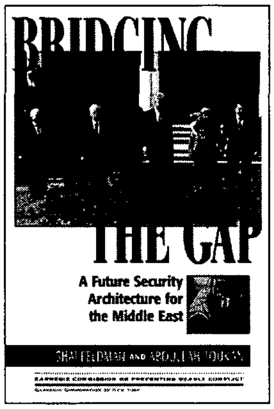

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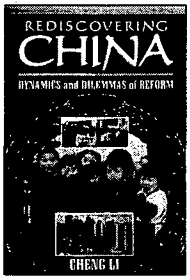

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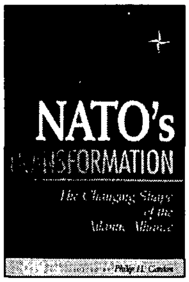

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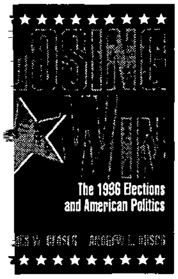

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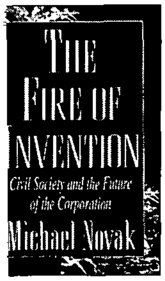

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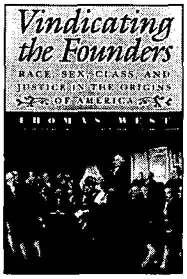

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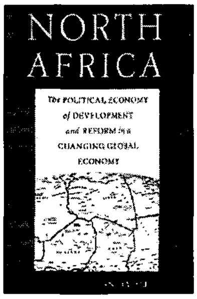
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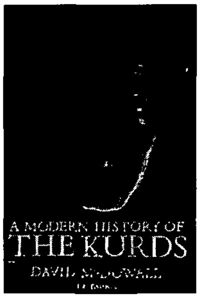

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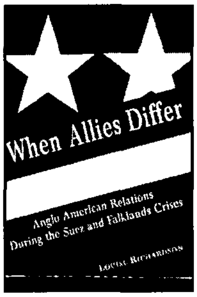
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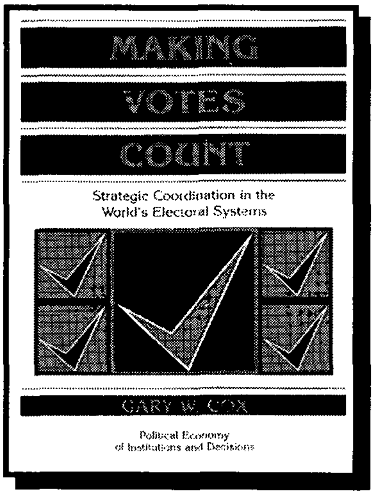
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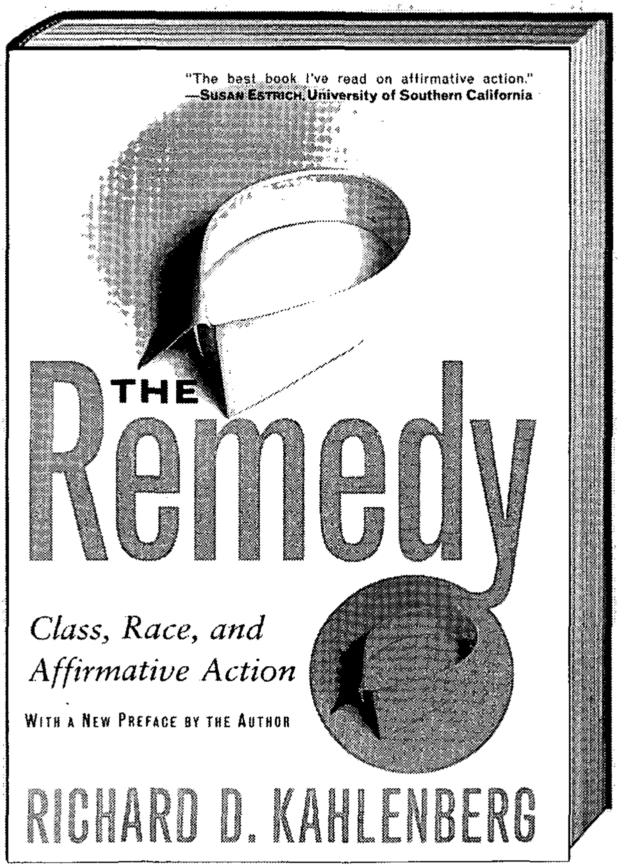
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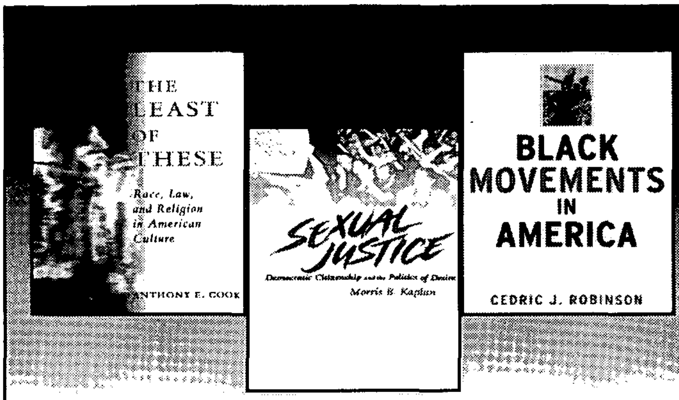
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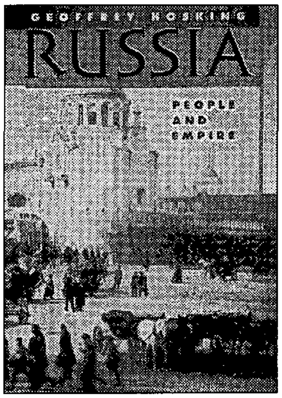

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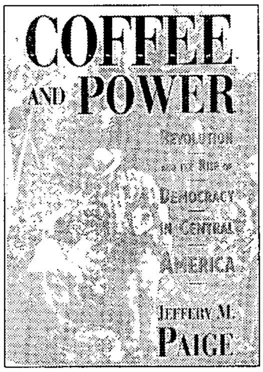

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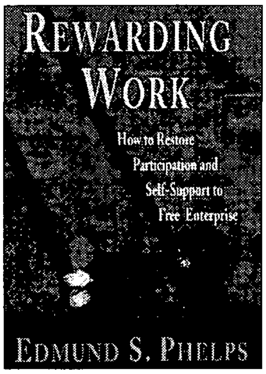
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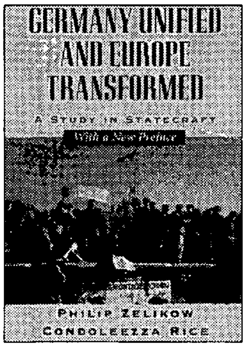

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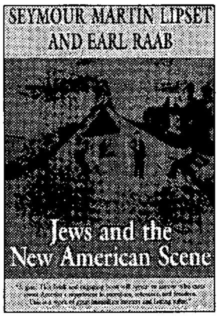
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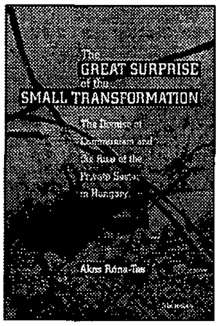
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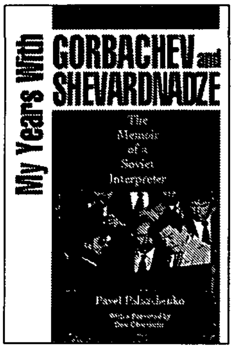
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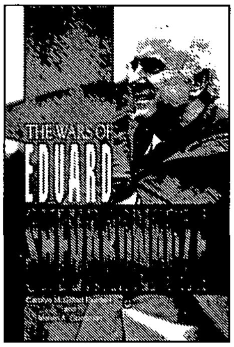
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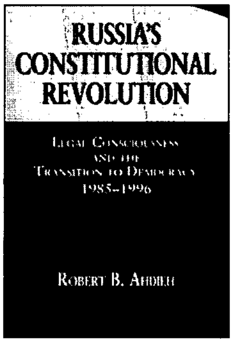
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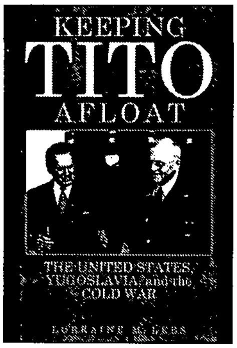
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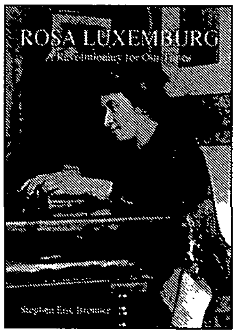
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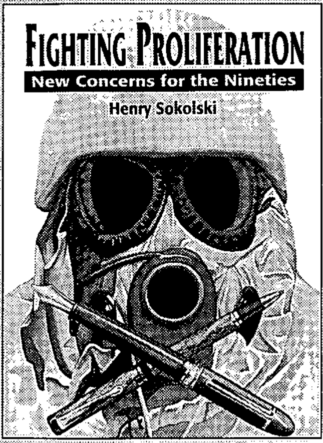

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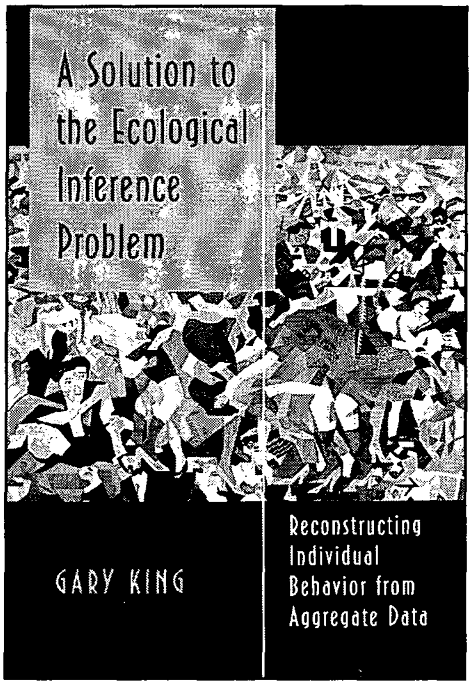
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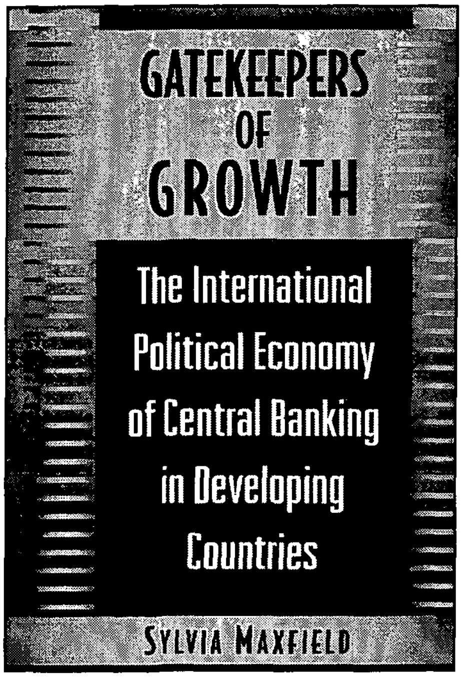

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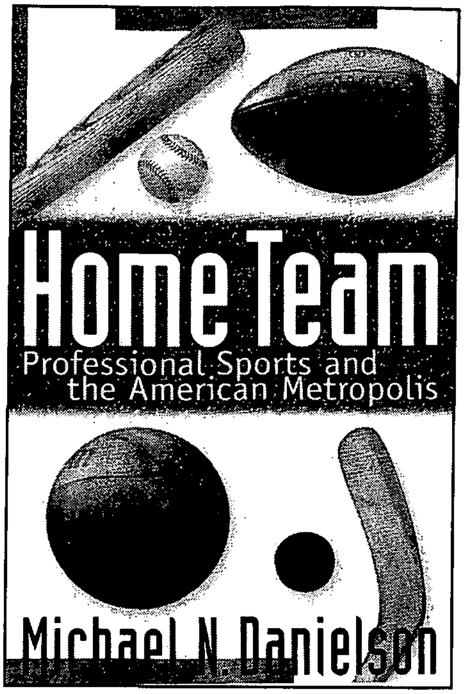
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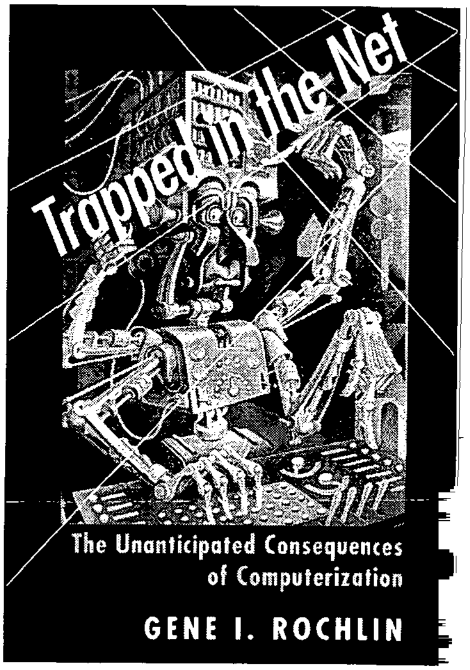
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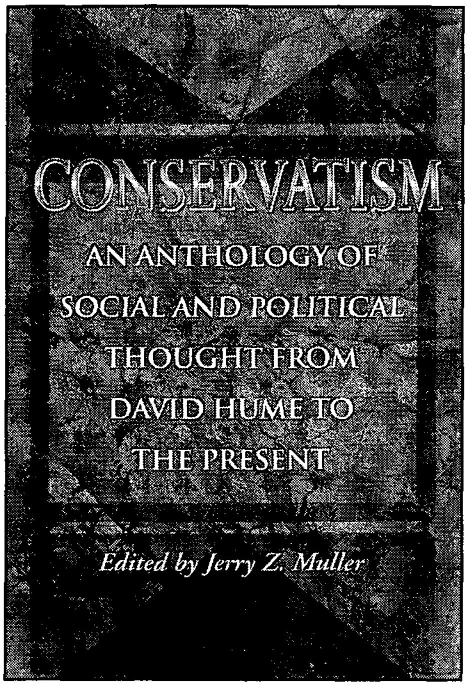

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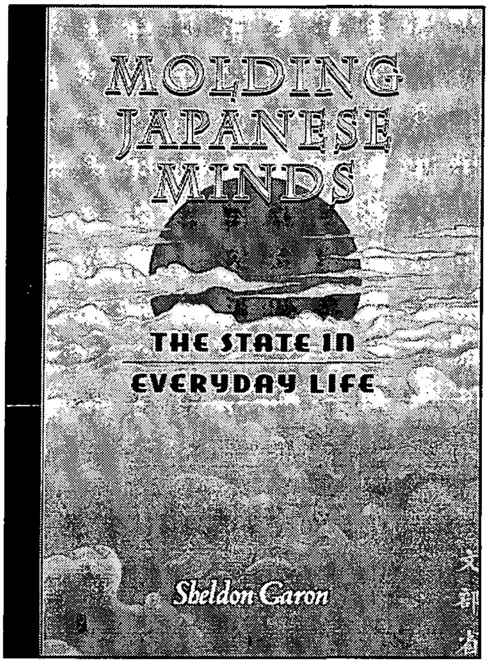

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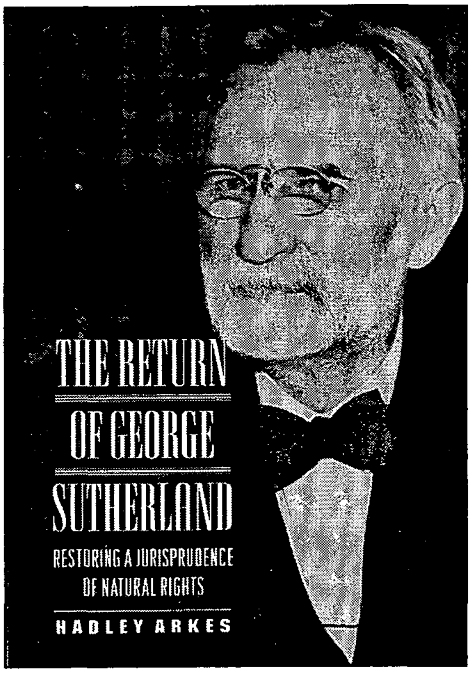

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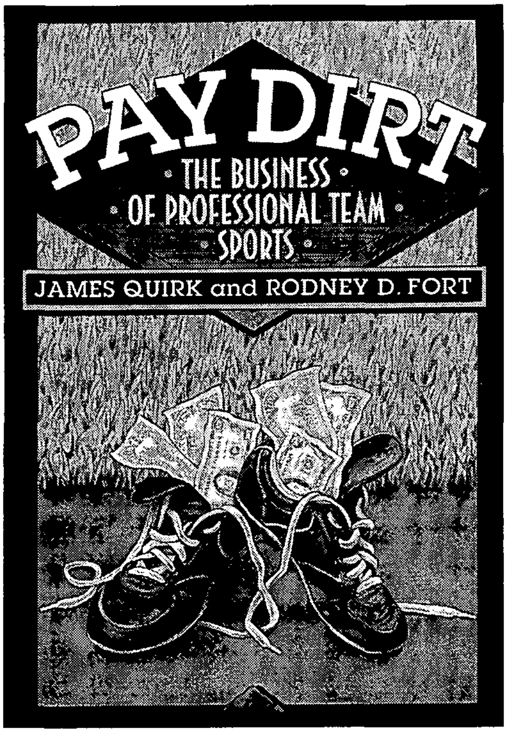

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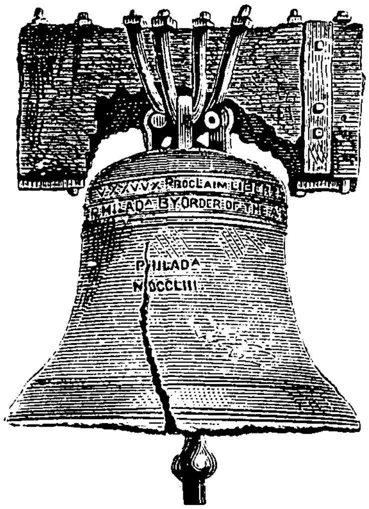

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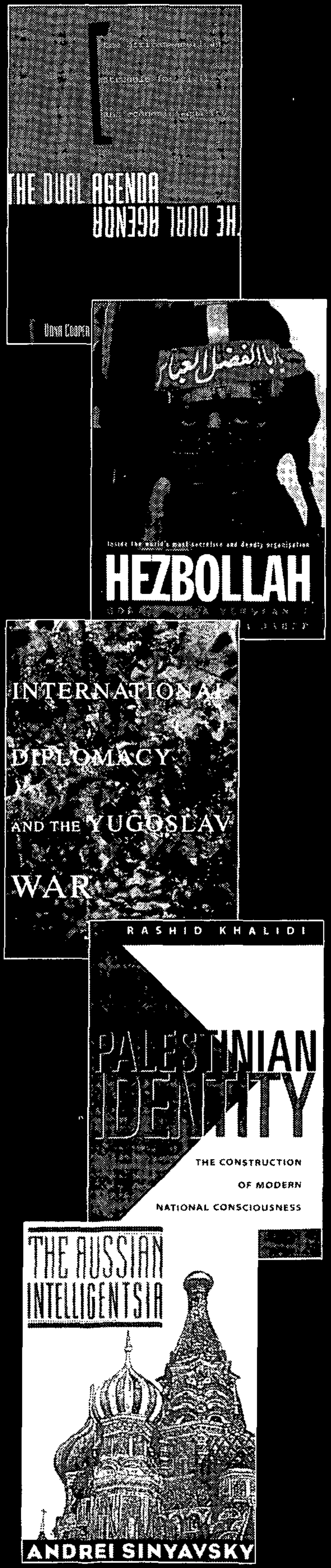
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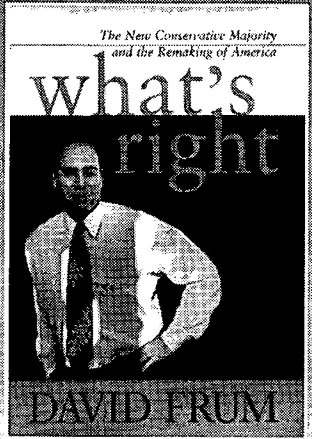

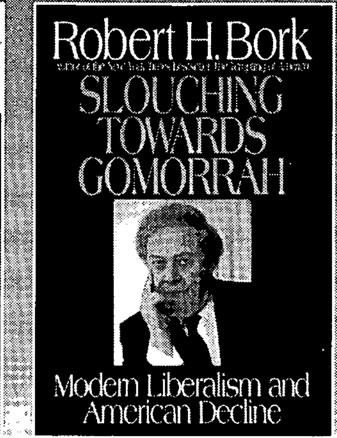
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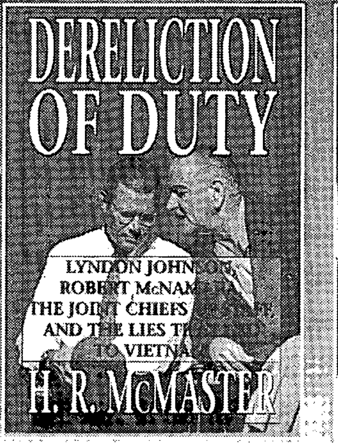
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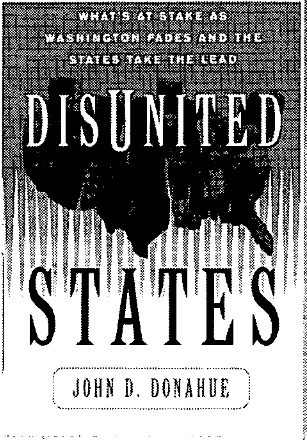
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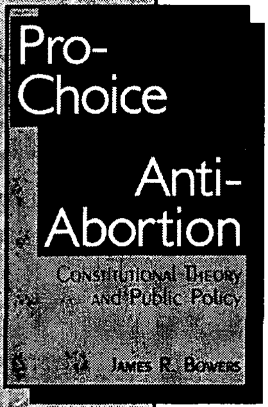

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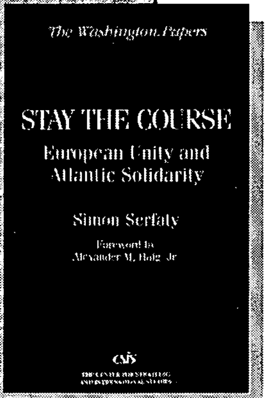
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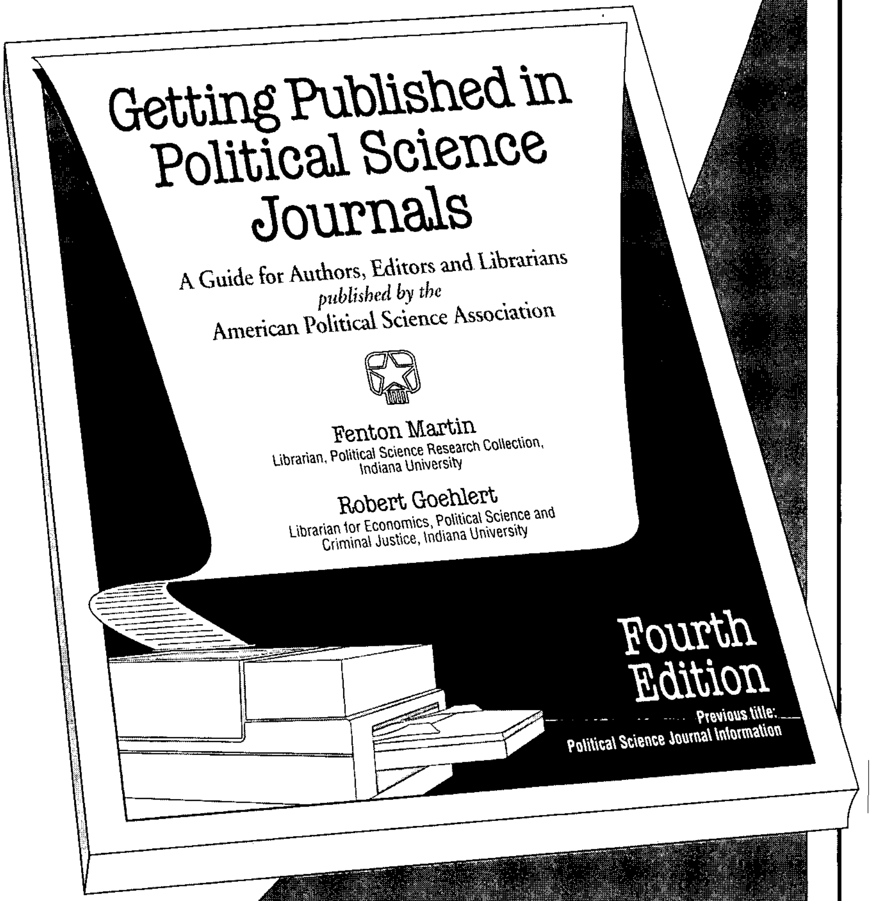

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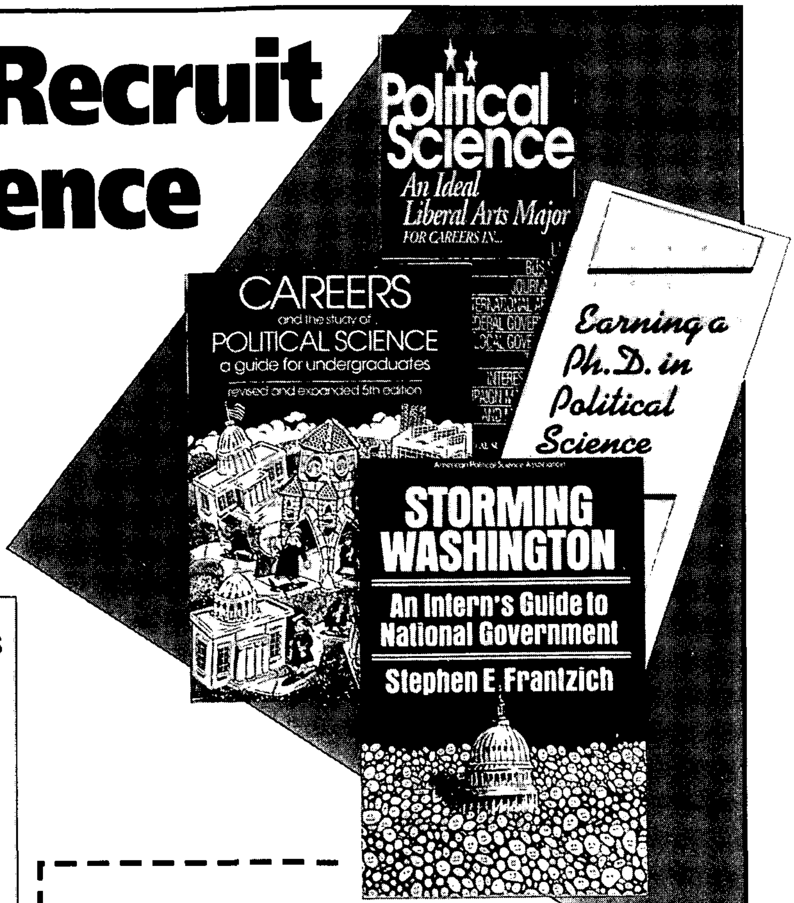
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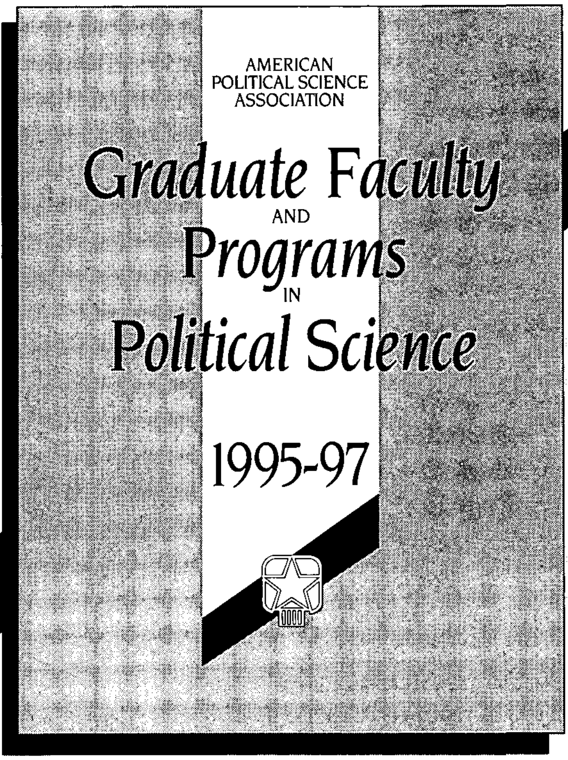

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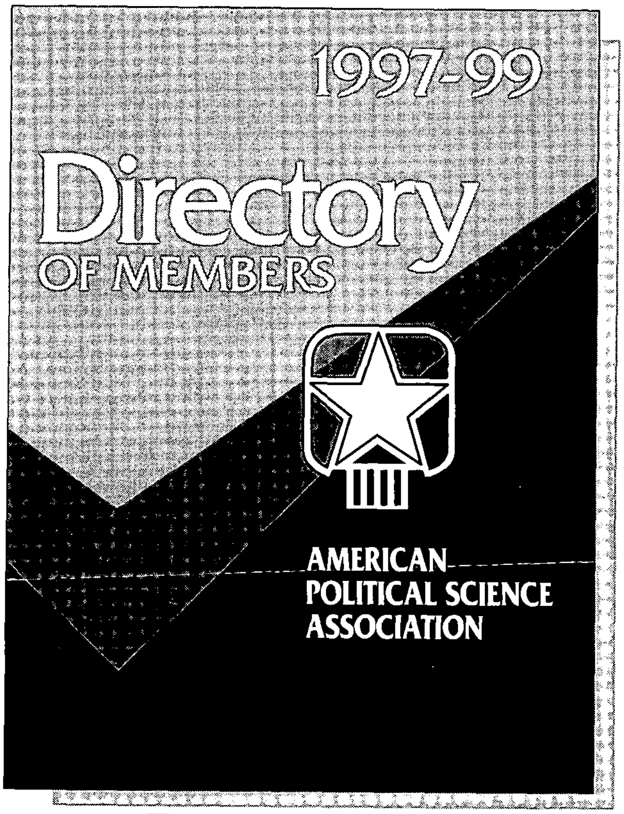

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